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Science Policy Study—Hearings Volume 20
RESEARCH FUNDING AS AN INVESTMENT

HEARINGS

BEFORE THE

TASK FORCE ON SCIENCE POLICY

OF THE

COMMITTEE ON
SCIENCE AND TECHNOLOGY
HOUSE OF REPRESENTATIVES

NINETY-NINTH CONGRESS

SECOND SESSION

APRIL 29, 30; MAY 1, 1986

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RESEARCH FUNDING AS AN INVESTMENT

TUESDAY, APRIL 29, 1986

House of Representatives,
Committee on Science and Technology,
Science Policy Task Force,
Washington, DC.

The task force met, pursuant to call, at 10:05 a.m., in room 2325, Rayburn House Office Building, Hon. Don Fuqua (chairman of the task force) presiding.

Mr. Fuqua. We begin a 3-day series of hearings on the subject of research funding as an investment.

Congress has frequently been advised that we should view the funding of scientific research as a long term investment, not as a current operating expense. In our hearings this week we will pursue that suggestion, focusing on the long-term payoff to society from research, and the feasibility of predicting and measuring that payoff.

As Dr. Bush stated, there are uncertainties in basic research, as there are in any business investments. Can such investments enable us to maintain our preeminent status in an ever changing world with its shifting technological environment? Some research projects may yield a payoff in new medicines or new technologies while others may not. While the payoff may come quickly, frequently the research investment will not come to fruition for 20 years.

How can we continue these investments in science prudently and intelligently while, at the same time, acknowledging that we owe an accountability to the American public? We look to these hearings for guidance on how to monitor and measure the returns.

The subject will be introduced by Ms. Nancy Naismith of the Office of Technology Assessment which prepared the report. "Research Funding as an Investment: Can We Measure the Returns?"

Our second witness will be Prof. David Mowery of Carnegie-Mellon. Professor Mowery collaborated on a commentary on "Traces" and "Hindsight", two studies that were made some years ago on measuring the contribution of research to innovation in the defense and private sectors, and, more recently, he has written on methodologies to make research and development more effective. Effective the day after tomorrow, he will be the study director for the National Academy of Sciences/National Academy of Engineering Panel on Technology and Employment.

Our third witness will be Ms. Mary Tanner from Shearson Lehman Brothers who will discuss the subject as it is viewed in the investment banking community, and, if we have time, Dr. Jack

Bagley will discuss the basic research investments from the point of view of a private research organization.

So we are delighted to have you back once again and would be pleased to hear from you this morning.

[A biographical sketch of Ms. Naismith and Dr. Andelin follows:]

NANCY CARSON NAISMITH

Nancy Carson Naismith is Program Manager for work in Science Policy, Transportation and Innovation at the Congressional Office of Technology Assessment [OTA]. OTA provides analysis of the positive and negative impacts of technology choices for the Congress.

Ms. Naismith has worked in the executive and legislative branches of the Federal Government since 1962. Her early work includes managerial responsibility in the areas of water quality, open space and neighborhood development, historic preservation, and recreation planning. Since joining OTA in 1977, Ms. Naismith has worked primarily in energy technology, with special emphasis in energy conservation and renewable technologies. She also served as the first vice president of the "Solar Bank."

Work completed under her direction includes a major study on civilian space stations, an analysis of high-speed rail, analysis of airport capacity problems for the coming decade, and an assessment of high technology and regional development. Current work in progress covers the transportation of hazardous materials, standardized measurement as a tool to evaluate educational policy, and three issues relating to science policy: regulatory forces, human resources, and science as an investment.

She holds a degree in general studies from the University of Washington, in Seattle, and a master's degree in public administration from the John F. Kennedy School of Government at Harvard, where she was a Littauer fellow.

DR. JOHN P. ANDELIN, JR.

Dr. John P. Andelin, Jr., is Assistant Director of the Congressional Office of Technology Assessment [OTA] for the Science, Information, and Natural Resources Division. Before his appointment at OTA, Dr. Andelin was the staff director for the Subcommittee on Advanced Energy Technologies and Energy Conservation Research, Development, and Demonstration of the Committee on Science and Technology, U.S. House of Representatives. He also served as a science consultant to that committee and as scientific adviser and administrative assistant to Congressman Mike McCormack (D–Wash.).

Dr. Andelin holds a Ph.D. in physics from the California Institute of Technology and a Master's degree in physics from Stanford University. He has been on the research staffs at Harvard University, Oak Ridge National Laboratory, and the Ford Scientific Laboratory.

STATEMENT OF NANCY CARSON NAISMITH, PROGRAM MANAGER FOR SCIENCE, EDUCATION, AND TRANSPORTATION, OFFICE OF TECHNOLOGY ASSESSMENT, WASHINGTON, DC; ACCOMPANIED BY DR. JOHN P. ANDELIN, JR., ASSISTANT DIRECTOR, SCIENCE, INFORMATION, AND NATURAL RESOURCES DIVISION, OFFICE OF TECHNOLOGY ASSESSMENT, WASHINGTON, DC, FOR DR. EUGENE FRANKEL, PROJECT DIRECTOR, OFFICE OF TECHNOLOGY ASSESSMENT, WASHINGTON, DC

Ms. NAISMITH. Thank you, Mr. Chairman. Good morning.

Mr. FUQUA. I might add that you're accompanied by Dr. John Andelin, too, who is not a stranger around this committee.

John, we're glad to have you back also.

Ms. NAISMITH. We would like to add that we regret that Dr. Frankel was unable to be with us to present the findings of the report. Dr. Andelin and I will do our best.

Mr. FUQUA. Thank you.

Ms. NAISMITH. In the fall of 1984, OTA was asked by the task force to provide information on the extent to which Federal research and development decisionmaking would be improved through the use of quantitative investment models. Let me emphasize that the assignment was not to try and analyze why we do science, or to try and sum the value of research, but to explore economic methods that might be of assistance in comparing alternative research efforts.

The assumption that federally-funded scientific research leads to economic benefits for the country has been fundamental to Government science policy since the end of World War II. Analysts have now abandoned the linear model of technological innovation that sees a simple progression from basic research to applied research to product development, but they still believe that scientific research plays a vital role in technological progress and consequently in economic growth. Economic returns, however, are neither the sole nor the primary purpose for Federal research spending.

Establishing the extent of return on investment in basic research is complicated by the fact that the relationship between basic research and innovation is often long term, indirect, and unpredictable. Moreover, the transformation of research into economically successful innovation depends not only on successful research but also on many factors in the economy which are completely outside the research process. These factors include the entire climate for investment, which includes Government policies on tax, regulation, and patents; the degree of competitiveness and entrepreneurialism in industry; the state of capital markets; foreign competition; wages, unionization, and other characteristics of the workforce. Even a highly successful basic research effort may never generate technological innovation or economic payoff if other factors in the economy are not conducive to technological change.

In the private sector, economists have developed techniques for selecting investments in situations that involve risk. These techniques include capital investment methodologies, portfolio analysis, and financial investment models, and all of these techniques do have heuristic properties that can be helpful in considering research and development investments. However, formal models make several assumptions that are not particularly helpful or appropriate for research decisionmaking, particularly basic research. They assume that the decisionmaker can estimate, in dollar values, the benefits from potential investments and know or estimate the probability of achieving the benefits. These assumptions are rarely applicable to Government-funded research, particularly basic research. In addition, formal models assume that the benefits of the investment return to, or are appropriable by, the investor. In contrast, the findings of most federally funded research are available to anyone to use, dispersing the benefits in a way that makes accurate accounting for them impossible.

While quantitative models of financial or other investment planning do provide methods for reducing risk, there exist virtually no economic models that can incorporate uncertainty. Research and development investment, especially basic research investment, is a classic example of investment undertaken under conditions of severe uncertainty. For this reason, quantitative models are likely to be far more useful in the evaluation of applied research and development or development decisions for exploratory research. In addition, quantitative models are more useful for decisions about distributing research and development investments among research installations or between performers in a single discipline than for allocating basic research funds across competing fields.

To determine the degree to which economic and noneconomic techniques are used by industry research managers, OTA reviewed the literature on use and interviewed experienced officials in both the public and private sector. The findings were somewhat surprising. In industry, where one might expect quantitative techniques to prevail due to the existence of a well-defined economic objective for the individual firm or business, OTA found great skepticism among research managers about the utility of such techniques. Managers found them to be overly simplistic, inaccurate, misleading, and subject to serious misinterpretation.

Industry managers recognize that attempts to link basic research activities directly and quantitatively to any kind of payoff—new products, profits, corporate image, internal consulting, scientific knowledge, or personnel recruitment—are flawed and of limited value. The uncertainties are too great, the causal paths too diffuse, the benefits too difficult to measure, and the timeframe too extended.

For project selection, OTA found that firms use standard economic return on investment techniques for projects at the development end of the cycle, where costs and benefits are generally well known and the risk can be quantified using past experience. At the basic research end of the spectrum, industry's project selection techniques tend to be subjective and informal, supplemented occasionally by simple scoring models. At the applied research or exploratory development stage, simple, unsophisticated selection procedures, based on a page or two of qualitative information or a simple rating scheme, dominate. Corporate managers have learned that research and development planning and budgeting is primarily an information and communication process, involving many persons and many levels of corporate hierarchy, using many criteria and often taking several iterations. Economic and financial modeling appear to play a secondary role in this process, serving primarily as inputs to overall corporate strategic planning.

OTA reviewed attempts to use econometric methods to measure the returns on Federal R&D dollars in three areas that have been well-studied: agriculture, aviation, and health. But all attempts have been limited in their ability to calculate specifically the benefits accrued as the result of Federal research and development investment.

OTA finds that the metaphor of research funding as an investment, while valid conceptually, does not provide a useful, practical guide for improving Federal research decisionmaking. Traditional economic return on investment calculations are inappropriate for evaluating prospective Government research expenditures for the following reasons.

First, studies of return on Federal research and development investment measure an average return on a total previous investment. They give little guidance as to the marginal return that can be expected from the next incremental investment in research and development, which is the decision that policymakers must make.

Second, most Government expenditures, including research and development expenditures, are for public goods whose market value is, by definition, extremely difficult to measure in economic terms. Nearly two-thirds of the Federal applied research budget is related to the production of public goods and services, such as health and defense. Improving productivity or producing an economic return are not primary justifications for most Federal programs.

Third, despite the success of retrospective studies of returns, there are no reliable formulae to relate future research and development expenditures to productivity improvements or other economic benefits. Predictions of future returns on investment cannot be made without such relationships.

To measure research output more directly, alternative indicators have been developed. Two principal approaches are now of interest. This includes bibliometrics, which evaluates research output based on scientific publications; and science indicators, which measure the vitality of the research enterprise in terms of degrees, personnel, awards, education, and similar indices.

The underlying assumption of the bibliometrics approach is that knowledge is the essential product of research, and publications are the most readily identified manifestations of that knowledge. Thus, bibliometrics measures the quantity of publications and citations by authors and research centers. Recently, other measurements and viewpoints have been incorporated by some students of bibliometrics to try and strengthen the method. While bibliometrics offers interesting potential for analysis, it is inherently retrospective. Bibliometrics provides no predictive capability, unless one believes that past performance is an indicator of likely future achievement, which is not an unreasonable assumption. Therefore, bibliometrics is more applicable to research program evaluation than to research planning. Second, it is not applicable to strategic decisions about resource allocations between fields. Most bibliometricians contend that the techniques can only be validly applied within individual disciplines, limiting their value for the most important decisions facing agency heads and congressional decisionmakers, allocating resources among fields.

Despite the limitations of bibliometrics, the National Science Foundation and the National Institutes of Health have undertaken extensive studies to refine the techniques and explore their applicability to research program evaluation. In addition, agencies of the French, Dutch, and British Governments, and the European Organization for Economic Cooperation and Development have applied some of these indicators to research programs in their countries.

OTA found that of all the Federal agencies supporting research and development, the National Institutes of Health conducts the most extensive ex post evaluation of its research through bibliometric studies and other activities carried out at individual institutes. The National Institute of Mental Health, for example, has begun to use bibliometrics and cluster groups to conduct a form of portfolio analysis. Looking at their program portfolios and the clustering of research publications by field, NIMH is identifying leading edges of research that might require more support from the Institute.

Another method of assessing the health of the research enterprise is to directly question the scientists and administrators involved in it through congressional hearings and agency advisory panels. The National Research Council carries out numerous reviews of research programs for the executive and legislative branches. The National Research Council, the National Science Foundation, and the National Institutes of Health cooperate regularly in the publication of "Science Indicators" which compiles information about research and development. Most lacking in the policy community at this time is a consensus on which indicators might be used, singularly or in combination, to measure the health of the research enterprise. A workshop or a report on the current views of the use of "Science Indicators" to measure the strength of the enterprise might be a useful first step in that direction.

Some observers argue that if economic returns are to be the primary measure of our research efforts, we should focus our attention as a nation on the factors that link science to technology and innovation. The United States spends less of its R&D budget than Germany, France, England, or Japan on research related directly to industrial productivity. Efforts to improve that situation could include an increased emphasis on technology transfer, increased support for generic research related to industrial needs, and adaptation of more focused forecasting and planning for industry-related research and development.

The OTA report describes R&D forecasting carried out by Japan's Ministry of International Trade and Industry. These forecasts identify research areas of long-term strategic importance using background information on research trends gleaned from industry, government, and academic reports from around the world. The process of forecasting provides a forum for people from different sectors and different professions to communicate about R&D priorities, encouraging coordination of research plans and the development of consensus. While the centralization of the Japanese R&D system has served Japan well, it is not clear that the pluralistic and decentralized system of R&D planning in the United States should attempt to imitate the Japanese system. Some analysts describe the greatest attribute of the U.S. system as its ability to "hedge against fluctuations in the fashions and policies influencing any line of R&D support." The dynamism of the U.S. system is generally agreed to be an essential element of our extraordinary achievements in research.

In summary, quantitative methods for measuring return on Federal research are of limited value. While there is no question of the importance of science to economic growth, existing economic methods have been developed for use in much more specific decision situations where results can generally be anticipated, costs measured with relative assurance, and returns can be assigned to the investor. Publicly-funded research decisions are more complex, are not motivated by nor designed for financial gain, and represent inherent uncertainty of outcome. While the investment metaphor is intuitively appealing, research represents investment in the future in a much broader sense than the narrow construction of economics.

Attempts at evaluating research decisionmaking should be analyzed in the context of the scale and structure of scientific activity in the United States. Much of the vitality of the American research system lies in its complex and pluralistic nature. Scientists, citizens, administrators, and Members of Congress all play various legitimate and important roles leading to final decisions on funding. While there may be ways to improve the overall process, reliance on economic quantitative methods is not promising. Expert analysis, which would include peer review, openness, experience, and considered judgment are better tools.

Thank you, Mr. Chairman.

[The prepared statement of Ms. Naismith follows:]

TESTIMONY OF DR. EUGENE FRANKEL

TO THE

HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY

SCIENCE POLICY TASK FORCE

April 29, 1986

Mr. Chairman, I am pleased to appear before you today to present the findings of OTA's technical memorandum, Research Funding as an Investment: Can We Measure the Returns? In Fall 1984 the Office of Technology Assessment was asked by the Task Force on Science Policy of the House Committee on Science and Technology to provide information on the extent to which Federal R&D decisionmaking would be improved through the use of quantitative investment models. Let me emphasize that the assignment was not to analyze why we do science, or to try and sum the value of research, but to explore economic methods that might be of assistance in comparing alternative research efforts. To carry out this study, OTA conducted a comprehensive search of the literature, met with numerous economists and analysts, and interviewed research decisionmakers in industry and government.

The assumption that federally-funded scientific research leads to economic benefits for the country has been fundamental to government science policy since the end of World War II. Analysts have abandoned the linear model of technological innovation that sees a simple progression from basic research to applied research to product development, but they still believe that scientific research plays a vital role in technological progress and consequently in economic growth. Economic returns, however, are neither the sole nor the primary purpose for Federal research spending.

Establishing the extent of return on investment in basic research is complicated by the fact that the relationship between basic research and innovation is often long-term, indirect, and unpredictable. Moreover, the transformation of research into economically successful innovation depends not only on successful research but also on factors in the economy that are completely outside the research process. These factors include the climate for investment; government tax, regulatory and patent policy; the degree of competitiveness and entrepreneurialism in industry; the state of capital markets; foreign competition; wages, unionization, and other characteristics of the work force. Even a highly successful basic research effort may never generate technological innovation or economic payoff if other factors in the economy are not conducive to technological change.

In the private sector, financial counselors and economists have developed techniques for selecting investments in situations involving risk. These techniques include capital investment methodologies, portfolio analysis, and financial investment models. All of these techniques have heuristic properties that could guide investment in R&D. However, formal models make several assumptions that are not particularly helpful to research decisionmaking. They assume that the decisionmaker can estimate in dollar values the benefits from potential investments and know or estimate the probability of achieving those benefits. These assumptions are rarely applicable to government-funded research, especially basic research. In addition, formal models assume that the benefits of the investment return to, or are appropriable by, the investor. In contrast, the findings of most federally-funded research are available to anyone to use, dispersing the benefits in a way that makes accurate accounting for them impossible.

While quantitative models of financial and other investment planning do provide methods for reducing risk, there exist virtually no economic models that can incorporate uncertainty. R&D investment, especially basic research investment, is a classic example of investment undertaken under conditions of severe uncertainty. However, as one moves from basic research to applied research and development, quantifiable risk replaces inherent uncertainty. **For this reason, quantitative models are likely to be far more useful in the evaluation of applied R&D or development decisions than for exploratory research. In addition, quantitative models are more useful for decisions about distributing R&D investments among research installations or performers within a single discipline than for allocating basic research funds across competing fields.**

In examining the applications of these models it should be kept in mind that they are only hypothetical constructs that attempt to describe complex events. Furthermore, the equations in the models reveal correlation, not causality. Nor do the models reveal the path by which R&D investment allegedly leads to productivity improvements. Moreover, most models treat R&D as the "residual" or the "thing that remains after everything else is accounted for", further weakening the proof of relationship, since it is entirely possible that other components of the residual exist, but have not been included in the analysis.

To determine the degree to which economic and noneconomic techniques are used by industry research managers, OTA reviewed the literature on use and interviewed experienced officials in both the public and private sector. The findings were surprising. **In industry, where one might expect quantitative techniques to prevail due to the existence of a well-defined economic objective for the individual firm or business, OTA found great skepticism among research managers about the utility of such techniques.** Managers found them to be overly simplistic, inaccurate, misleading, and subject to serious misinterpretation.

OTA also found little systematic data about the use of quantitative techniques. Most articles describe a process adopted by one firm or another without any indication as to how widespread the practice in industry as a whole.

Industry managers recognize that attempts to link basic research activities directly and quantitatively to any kind of "payoff" - new products, profits, corporate image, internal consulting, scientific knowledge, or personnel recruitment - are flawed and of limited value. The uncertainties are too great, the causal paths too diffuse, the benefits too difficult to measure, and the time frame too extended. Basic research usually represents a small fraction (5 to 10 percent) of private sector R&D budgets, and is not subject to the same type of financial scrutiny as applied research and development activities.

For project selection, OTA found that firms use standard economic return on investment techniques for projects at the development end of the cycle, where costs and benefits are generally well known and the risk can be quantified using past experience. At the basic research end of the spectrum, industry's project selection techniques tend to be subjective and informal, supplemented occasionally by scoring models. At the applied research or exploratory development stage, simple, unsophisticated selection procedures, based on a page or two of qualitative information or a simple rating scheme, dominate. **Corporate managers have learned that R&D planning and budgeting is primarily an information and communication process, involving many persons and many levels of corporate hierarchy, using many criteria and several iterations. Economic and financial modeling appear to play a secondary role in this process, serving primarily as inputs to overall corporate strategic planning.**

Government R&D managers also avoid quantitative techniques for project selection although some, most notably at NIH, use certain quantitative techniques to assist in program evaluation. OTA reviewed attempts to use econometric methods to measure the returns on Federal R&D dollars in three areas that have been well-studied; agriculture, aviation, and health. But all attempts have been limited in their ability to calculate specifically the benefits accrued as the result of Federal R&D investment.

OTA finds that the metaphor of research funding as an investment, while valid conceptually, does not provide a useful practical guide to improving Federal research decisionmaking. Traditional economic return on investment calculations are inappropriate for evaluating prospective government research expenditures for the following reasons:

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>
> Third, despite the success of retrospective studies of returns, there are no reliable formulae to relate future R&D expenditures to productivity improvements or other economic benefits. Predictions of future returns on investment cannot be made without such relationships.

A major problem with the use of economic models in research decisionmaking is that they deal with economic "indicators" that are at best indirectly influenced by research. To measure research output more directly, alternative "indicators" have been developed by students of science policy over the past two decades. Only recently have these methods begun to be considered seriously by research policymakers as possible aids to decision processes. The two principal approaches now of interest are bibliometrics, which evaluates research output based on scientific publications; and science indicators, which measure the vitality of the research enterprise in terms of degrees, personnel, awards, and education.

The underlying assumption of the bibliometrics approach is that knowledge is the essential product of research, and publications are the most readily identified manifestations of that knowledge. Thus, bibliometrics measures the quantity of publications and citations by authors and research centers. Recently, other measurements and viewpoints have been incorporated by some students of bibliometrics, to try and strengthen the method. While bibliometrics offers interesting potential for analysis, it is inherently retrospective. Bibliometrics provides no predictive capability, unless one believes that past performance is an indicator of likely future achievement — not an unreasonable assumption. **Therefore, bibliometrics is more applicable to research program evaluation than to research planning. Second, it is not applicable to strategic decisions about resource allocations between fields. Most bibliometricians contend that the techniques can only be validly applied within individual disciplines, limiting their value for the most important decisions facing agency heads and congressional decisionmakers — allocating resources among fields.**

Despite the limitations of bibliometrics, NSF and NIH have undertaken extensive studies to refine the techniques and explore their applicability to research program evaluation. In addition, agencies of the French, Dutch, and British governments, and the European Organization for Economic Cooperation and Development have applied some of these indicators to research programs in their countries.

OTA found that of all the Federal agencies supporting R&D, NIH conducts the most extensive ex post evaluation of its research through bibliometric studies and other activities carried out at the individual institutes. The OTA report includes a section on the use of bibliometrics at NIH and concludes that in almost all cases, bibliometrics studies were used to evaluate program performance and conformity to agency or institute mission. In some cases, they helped to identify areas for future funding. The National Institute of Mental Health, for example, has begun to use bibliometrics and cluster groups to conduct a form of "portfolio analysis." Looking at their program portfolios and the clustering of research publications by field, NIMH is identifying leading edges of research that might require more support from their institute.

One method of assessing the health of the research enterprise is to directly question the scientists and administrators involved in it through congressional hearings and agency advisory panels. The National Research Council carries out numerous reviews of research programs for the executive and legislative branches. NRC, NSF, and NIH cooperate in the regular publication Science Indicators which compiles information about levels of funding devoted to R&D by source, sector, nature of work, and performer, as well as statistics on the distribution of scientific and engineering personnel, graduate students, and support for graduate education and training. The "indicators" can only supplement, and not replace, informed peer judgment of the scientific process. But they can help complete the anecdotal, fragmentary, and necessarily somewhat self-interested picture of the state of science presented by the researchers themselves. Most lacking in the policy community is a consensus on the indicators that might be used singly or in combination, to measure the health of the research enterprise. A workshop or report on the use of Science Indicators to measure the health of the research enterprise might be a useful first step in that direction.

Some observers argue that if economic returns are to be the primary measure of our research efforts, we should focus our attention, as a Nation, on the factors that link science to technology and innovation. The United States spends less of its R&D budget than Germany, France, England, or Japan on research related directly to industrial productivity. Efforts to improve that situation could include an increased emphasis on

technology transfer, increased support for generic research related to industrial needs, and adaptation of the more focused forecasting and planning for industry-related R&D.

The OTA report describes the R&D forecasting carried out by Japan's Ministry of International Trade and Industry (MITI). These forecasts identify research areas of long-term strategic importance using background information on research trends gleaned from industry, government, and academic reports from around the world. The process of forecasting provides a forum for people from different sectors and different professions to communicate about R&D priorities, encouraging coordination of research plans and consensus. The centralization of the Japanese R&D system has served Japan well in applying research findings. **It is not clear that the pluralistic and decentralized system of R&D planning and budgeting in the United States should attempt to imitate the Japanese system.** Some analysts describe the greatest attribute of the U.S. system as its ability to "hedge against fluctuations in the fashions and policies influencing any line of R&D support." The dynamism of the U.S. system is generally agreed to be an essential element of our extraordinary achievements in research.

In summary, quantitative methods for measuring return on Federal research are of limited value. While there is no question of the importance of science to economic growth, existing economic methods have been developed for use in much more specific decision situations, where results can generally be anticipated, costs measured with relative assurance, and returns can be assigned to the investor. Publicly-funded research decisions are more complex, are not motivated by nor designed for financial gain, and represent inherent uncertainty of outcome. While the investment metaphor is intuitively appealing, research represents investment in the future in a much broader sense than the narrow construction of economics.

Attempts at evaluating research decisionmaking should be analyzed in the context of the scale and structure of scientific activity in the United States. Much of the vitality of the American research system lies in its complex and pluralistic nature.

Scientists, citizens, administrators, and Members of Congress all play various roles leading to final decisions on funding. While there may be ways to improve the overall process, reliance on economic quantitative methods is not promising. Expert analysis, openness, experience, and considered judgment are better tools.

DISCUSSION

Mr. FUQUA. Thank you very much.

The OTA study notes that there's a correlation between industry, and research and development expenditures and productivity growth, and that there is only a weak or nonexistent link between the Government R&D expenditures and industry's productivity growth. How do you explain that significant difference?

Ms. NAISMITH. A number of economists who are following me may wish to explain that in more detail, but as we understand it, it has largely to do with the fact of the Federal investment being a public good so that the indices used to measure return on investment are difficult to apply.

As the study points out, the analysts who have attempted to measure the return find an extremely wide range of return from quite positive to sometimes even negative returns, and we think this has to do with the fact that in industry the actual return can be directly and specifically measured to the firm or at least to the industry, whereas the benefits and the clear productivity from Federal research are so dispersed through the society that they are extremely difficult to trace and assign. We think it speaks to the problem of the methodology as opposed to the issue of whether or not science is productive.

Mr. FUQUA. Well, if basic research takes sometimes 15 to 20 years to reach into the marketplace, then how do we, on a short-term basis, monitor the value of our investments, and what are the indicators? Is it GNP, or what do we use as a guide? Are we just stabbing in the dark?

Ms. NAISMITH. I wouldn't say that we're stabbing in the dark. I would say that as we begin to think in different fields about the type of objectives that are stated and that are pursued, there might be other ways to look at whether or not those objectives are obtained.

In the public health areas, for instance, there are measurable steps that are made and in progress in research—understanding disease, for instance, and understanding the biochemistry behind it. Those things are measurable and identifiable, and to a certain extent, in any given area, program evaluation and retrospective work does give a measure of whether or not progress is being made.

There are a number of things which the committee might want to consider, some of which are suggested in the report for purposes of identifying whether or not the funding is being used most appropriately. There might be some value to the committee in asking people who come and discuss funding approaches to provide analysis based on some of the bibliometrics techniques on applying some of the science indicators. It may also be that in other questions, other types of measurable progress could be identified before the program is undertaken.

Mr. FUQUA. But when we take a bill to the floor of the House, we have to convince our colleagues, because we all must be accountable to the general public of how we spend Federal moneys. Now how do we try to, or how can we, make sure that there's an accountability of our expenditure of tax dollars? Are there ways that you can demonstrate accountability, that this has been of value?

You know, people periodically wave some report about some silly-sounding name, that gets a great big belly laugh, of some research project. Maybe it has meritorious work, just a poor title, but it's ridiculed, and I'm not talking about the ridicule, but how do we point out the accountability to the American public? Is there any measure?

Ms. NAISMITH. The measure, I think, is certainly not a short-term economic return on the dollar. It has to be an extended measurement over time. I think it is valid to include contributions to fundamental knowledge and the development of our technological base, which includes the population of trained and educated people.

Mr. FUQUA. Which a lot of those are intangible benefits.

Ms. NAISMITH. That's right, they are, and it's a very difficult problem for the Congress. It is like priority setting in many fields, where it is very difficult to say the extent of the commitment, the importance of the commitment itself, and that is one of the burdens that the committee faces in making these choices. I'm afraid we don't have an answer for you, Mr. Chairman.

Mr. FUQUA. That's been one of our problems.

Well, can you compare the research position of other countries? Are there signs or indicators that determine areas of need to direct Government support? Can you say that certain countries in Europe are better off, or the Soviets, or Japanese, and correlate it to the amount of money that they put into research and reach some measure or some indicators that will do that?

Ms. NAISMITH. I think there is no question but that our scientific enterprise is substantially more robust, more dynamic, and stronger than any other scientific enterprise. There is some issue as to the question of competitiveness—international global competitiveness—and to the extent that that is the concern facing the Congress, then there need to be measurements based on the questions of technology transfer, and looking at how well the applied research portion of the budget is handled and managed.

Many analysts feel that there is a problem in this country in translating from this enormous and very vital science base into the technological base. This seems to be a difficulty for us, and it's something that in this country we have determined is best left to the private sector where in other countries, they have determined that it is in their national interest, particularly in Japan and the Western European countries, to try to form consensus, select targets, and proceed in a somewhat more organized manner than we do, and to the extent that that is the principal concern facing the committee, looking for measurements in those areas I think might be quite useful.

Mr. FUQUA. Maybe if we could develop better econometric models, that could help give us a better feel for the indicators of milestones that we've met.

Ms. **Naismith**. Possibly.

Mr. **Fuqua**. Mr. Slaughter.

Mr. **Slaughter**. Do you have any comments on whether we are making the approximately proper division of research support between the academic community and the industrial sector of the economy?

Ms. **Naismith**. It seems to be working. We have some serious problems in the university sector that relate to facilities and the provision of facilities, which is to some extent a separable problem.

In order to do research, and particularly in many fields today, it's necessary to have equipment which is not available in many of our universities, because there's been a long period of inadequate funding for capital investments. Now universities have tried to resolve these problems in different ways. Some feel that the university sector could be substantially more productive if measures could be found to improve the investment in laboratories, facilities, and instruments.

But we see a growing sense of new institutions being developed between industries and universities. Industries seem to be taking more responsibility for trying to provide assistance to universities for basic research in ways that are acceptable to universities and do not compromise the traditional integrity of the university. We see this as quite promising, and the development of these new institutional links and new kinds of lifetime reeducation teaching for scientists and engineers—we think this is an indication of the dynamic system that we have in the country, and that the country seems to be responding to budgetary pressures through making new accommodations.

Mr. **Slaughter**. I gather that the United States lead in basic research today among all the countries in the world is sort of a reversal of what it was a couple of generations ago. I have no more questions.

Mr. **Fuqua**. I might point out that—has there been any question of the scientific basis for the bibliometric approach? I notice an article in the New York Times recently where that was attacked.

Ms. **Naismith**. I think that it is—certainly there are questions about bibliometrics, and there were applications of the bibliometrics techniques to some of the activities at CERN and other research centers in the last few years that were defined by some as perhaps validating views that people had—decisions that had already been made about CERN.

There is the concern particularly that, as we pointed out, it is not a predictive technique; it is very retrospective, and it also overlooks the fact that science is a loose amalgam, confederation, of people working together and exchanging information back and forth, so that trying to do a cost-effective analysis of a facility in terms of its publications is a technique that many scientists feel is inappropriate. So I think there is some concern in the science community about this.

The students of bibliometrics are trying to refine their techniques and see if they can come up with something that would be more acceptable in the scientific community. They, too, are trying to answer this question that the committee is grappling with, to try to give better indicators of what they call the science of science, but there has been some discomfort with those techniques.

Mr. **Fuqua**. Thank you very much for joining us this morning.

Ms. **Naismith**. Thank you, Mr. Chairman.

Mr. **Fuqua**. Thank you, John.

[Questions and answers for the record follow:]

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June 24, 1986

6 1986

The Honorable Don Fuqua
Chairman
Committee on Science and Technology
U.S. House of Representatives
Washington, DC 20515

Dear Don:

Thank you for your letter of June 10, requesting some additional information in connection with our Technical Memorandum on Research Funding as an Investment. We are pleased to provide this information, which has been prepared by staff in our Program on Science, Education, and Transportation.

1. In the oral testimony, reference was made to the use of bibliometrics and cluster groups. It was stated that, "NIMH is identifying leading edges of research that might require more support from the institution by the use of these techniques." How can clustering be interpreted to make such identification? This only tells numbers of papers, not content nor perhaps more importantly, the value of the content.

Response: Cluster maps of articles and publications do indicate content, and it is important to remember that the papers are being published in refereed journals. If one accepts the notion that refereed journals screen out low quality research, then clustering indicates some level of quality in the research underway. NIMH looks at clusters as an indication of where activity is. This allows them to review their own research portfolio in relation to where the overall field is concentrating effort. Thus, cluster analysis serves as a gauge of the relevancy of NIMH's funding patterns to field interests. Decisions as to the value of the research identified in this manner would still be a matter of judgment.

2. Can you comment, based on your study, on how effective government programs are in meeting the research needs of industry? Is there precedent or practice to conclude that the purpose of federally funded programs is to satisfy the needs of industry?

Response: In general, as our report points out, the Federal research activities, particularly in basic research, are not designed to satisfy the needs of industry. In fact, the basis for much Federal research is the belief that the type of long-term, high-risk activity appropriate for Federal support is not likely to be funded by industry, or in fact may not have immediate application for industry. Research with commercial utility is assumed to be the responsibility of the private sector. Notable exceptions to this policy include research in aeronautics and aerospace, agriculture, and health. As pointed out in our study, research in these areas has historically been defined as effective, although returns are difficult to measure. Attempts to assist commercial research in other sectors, for example energy and housing, have been more mixed.

A substantial amount of work done at OTA over the past few years has pointed out that, in comparison to other countries, the United States does not have a strong connection between industrial research needs and Federal research funding. Because most American industry does not fund much basic research, some students of industrial policy believe that this results in a comparative disadvantage to American industry. The steps taken by the National Science Foundation during the past two years to provide increased support for engineering research is one example of an attempt to alter this situation. Other examples include continuing attempts to improve the transfer of research from the laboratory to the private sector.

I hope that this information is helpful to the Task Force. Please feel free to contact me or Nancy Naismith, Program Manager, on 6-2214, if there are any questions.

Sincerely,

Jack

John H. Gibbons

Mr. **Fuqua**. Our next presenter will be Prof. David C. Mowery of the social sciences department at Carnegie-Mellon University.

We are pleased to have you this morning.

STATEMENT OF DR. DAVID C. MOWERY, ASSOCIATE PROFESSOR OF ECONOMICS AND SOCIAL SCIENCES, CARNEGIE-MELLON UNIVERSITY, PITTSBURGH, PA, AND RESEARCH FELLOW, CENTER FOR ECONOMIC POLICY RESEARCH, STANFORD UNIVERSITY, STANFORD, CA

Dr. **Mowery**. Thank you. I'm pleased to be here.

I have a somewhat lengthy statement which I propose to submit and summarize in my oral remarks.

Mr. **Fuqua**. We have a rule here: We'll let it be made part of the record, providing the summary is shorter than it would take to read the entire statement.

Dr. **Mowery**. I certainly hope it will be.

Mr. **Fuqua**. Thank you.

Dr. **Mowery**. I'm not sure my voice could last through the reading.

I find myself very supportive and in general agreement with the conclusions of the OTA report on the limited relevance of quantitative techniques for R&D decision making, and I might add that I agree, second, with Mr. Naismith's comments on the difficulty of establishing a unidimensional measure of the rate of return to basic research investments by the Federal Government.

Quantitative methods for the analysis of basic research investment are not widely used in industry, and this, I think, reflects the fact, among other things, that the quantitative methods available from other areas, such as minerals exploration and investment—financial investment—really are designed to deal with and minimize and manage risk rather than uncertainty. Uncertainty is really the characteristic that pervades basic research as opposed to development or applied forms of research.

However, I think there are lessons from the management of successful industrial research labs that concern the process of decisionmaking within the executive branch and the Congress on research priorities within a given research budget.

So I think that really the lessons from industrial research management for purposes of R&D decisionmaking really concern process rather than specific techniques, and what I propose to do is briefly try to distinguish among several levels of administration on research in order to clarify and provide some context for my comments on this general topic; I will follow that with a brief—very brief discussion of the relevance of the "Hindsight" and "Traces" studies which were done in the late 1960's to the concerns of this task force on the returns of the R&D investment.

It seems to me that there are really three broad categories of R&D administration as we proceed from a very high level of aggregation to a level of considerable detail. At the most aggregate level, a decision is made, explicitly or not, on the share of the total budget devoted to basic research. This decision incorporates a tradeoff between the benefits derived from the Federal investment in basic research and the benefits derived from Federal expenditures on other programs—income maintenance, defense, and a wide range of other activities. This is not a decision on which economists or many other individuals can provide much by way of useful advice. These decisions ultimately are political in the best sense of the term. There really exists no objective calculus on the basis of which we can make these decisions.

A second level, an intermediate level, of administration with respect to research and development investment deals with the establishment of priorities and the allocation of the basic research budget among disciplines or areas of research, and this area, I think, is really the one to which the OTA report is addressed. It's perhaps one that has been a central concern of this task force; and, as the OTA report suggests, quantitative measures really do not help us very much in establishing and defending priorities and allocations among areas of basic research investment. As I say, this is reflected in the fact that industry rarely employs these techniques or administration for the allocation of its basic research budget among various disciplines in areas of research. However, I think this area is one in which the process by which industry makes these decisions—the management of industrial research labs, the management of corporations investing heavily in basic research—the process differs substantially from that which we observe in the executive branch and in Congress.

In the most successful industrial research laboratories, we see a consistent effort to enforce competition among all areas—potential fields of inquiry in basic research for the basic research dollar. That is to say, we see in the most successful industrial research labs the enforcement of competition for the marginal basic research dollar among all potential areas of inquiry and the pursuit of a comprehensive review of basic research priorities at a fairly high level.

Now I would propose, Mr. Chairman, that within both the executive branch and in Congress we really do not see a venue at either end of Pennsylvania Avenue within which this form of comprehensive review of the full range of basic research programs can be undertaken. Within the executive branch, the Office of Science and Technology Policy has conducted a series of reviews with, in many cases, the aid of the National Academy of Sciences' Committee on Science, Engineering, and Public Policy in a series of research briefings; also the Office of Management and Budget in the executive branch conducts an R&D cross cut, as they call it. But neither one of these really are linked to senior level policymakers, nor are they well integrated into the resource allocation process within the executive branch.

Within Congress, obviously, fragmentation in the administration process is also very apparent. Defense basic research is treated separately from basic research expenditures in other areas, and even within the civilian sector, basic research investment decisions are made by a wide range of committees and involve a fairly fragmented and incremental process of consideration of priorities. We don't see the sorts of comprehensive review of the full range of potential basic research activities in the Federal Government that we observe in most successful industrial research enterprises.

I would suggest that in both the executive branch and the Congress the process might be changed to enforce or support more by way of a comprehensive review. Within the executive branch, the research briefings could be strengthened; the OMB cross-cutting review could be altered to involve senior policymakers and linked more explicitly to the resource allocation process within the executive branch. Within the Congress, potentially—and I propose a couple of tentative suggestions—a select committee of basic research priorities could act as a clearing house for research briefings from the National Academy or other sources and serve in an educative role. Alternatively, a subcommittee of the House or Senate Budget Committees could be established.

However, the specifics of changes in the process, I would suggest, are less important than the general principle, which is that we will reach more defensible and perhaps better decisions by altering the process for this level of administration, that is to say, decisions on priorities within the basic research budget.

Finally, there's a third level of administration with respect to basic research investment, one that operates at a level of considerable detail, and this concerns the allocation of basic research funds among facilities or research teams operating within a given discipline. This is an area, clearly, in which Congress cannot be involved on a consistent or daily basis simply because of the welter of detail that these decisions involve. However, a number of congressional decisions—particularly those involving capital expenditures on research facilities and the like—do involve favoring or providing resources for one research team or facility over another. I would suggest that at this level in oversight, and perhaps in some of these detailed decisions in which Congress is involved as well as the executive branch, efforts might be made to employ more of the bibliometric or patent-based indices of research performance. As Ms. Naismith suggested, these are best suited to comparisons among research teams and facilities operating within a single discipline. They do not operate well across disciplines, but they have some relevance within a single discipline, and I would suggest that initiatives might be undertaken to try to integrate these more into oversight hearings and the like within the Congress, and obviously within the executive branch.

I would finally suggest, Mr. Chairman, that one of the most disturbing aspects of the Gramm-Rudman-Hollings bill is the fact that the allocation procedures embodied in Gramm-Rudman really move away from any possibility for this comprehensive review and potential reallocation of basic research funds within the basic research budgets. Gramm-Rudman-Hollings enshrines fragmentation and incrementalism, if you will, to some extent, and I view that as an unfortunate consequence, although I recognize the severity of the problem to which it is addressed.

Let me now turn briefly to "Hindsight" and "Traces," which were done in the late 1960's, trying to measure the contributions to innovation of research, of individual research discoveries, or events, as they were called.

"Hindsight" was a study commissioned by the Department of Defense, the then-Director of Defense Research and Engineering, to evaluate the innovation process lying behind weapons system.

"Traces" examined the scientific discovery process which led to a series of commercial innovations.

It's interesting, I think, and important to note that neither of these studies succeeded—"Hindsight" attempted, "Traces" did not—in computing measures of the return on the investment in the research that led to these innovations. "Hindsight," indeed, had this as a central goal, but the study ultimately abandoned this attempt. I think the central reason for this is very interesting, and I quote from the study: "We rejected the possibility that any simple or linear relationship exists between cost of research and value received."

I think that statement encapsulates many of the difficulties that we as economists and you as policymakers encounter in trying to trace returns from the investments in research. These events are complex, they are interlinked in a complicated way, and it is very difficult to add up individual discoveries and attribute a share of the eventual value of the innovation or one or another, or even three or four, of these discoveries; it's simply very difficult. Discoveries spillover into other innovations; they ramify across fields. "Hindsight" could not do it; "Traces," as I say, did not attempt it.

Do these studies contain other useful principles or analytic techniques for administration in R&D? I would suggest that the lessons of these studies for this task are very limited. They are exclusively retrospective in focus, and therefore do not provide much by way of predictive capability. It's nice to know how you got here, but it doesn't necessarily tell you where to go from here.

There are some other problems, some obvious ones. There are great differences, and here I'm talking primarily about the "Hindsight" study. The relevance of defense research and engineering which operate in an essentially non-market environment to Federal funding of exclusively basic research and the impact of that basic research on innovations in the commercial marketplace, is limited. Defense research and engineering is a very different animal both in the characteristics of the programs funded by the Federal Government and, in the characteristics of the markets that the products or the results of those programs face than are the research programs or the outcomes of research funding in other areas.

The conclusions of both studies are also extraordinarily sensitive to fairly arbitrary assumptions. "Hindsight" imposed a 20-year cutoff on the relevant research events for examining the contributions of research to weapons systems innovation, and "Hindsight" concluded that basic research made a very minor contribution to innovation in weapons systems. "Traces," on the other hand, did not impose a comparably short cutoff and concluded quite the opposite—basic or non-mission-oriented research contributed enormously to innovations. So where you come out depends, in these studies, very essentially on how far back you go in time in looking for contributions to innovation. I don't think either study is completely correct. I think this simply demonstrates the sensitivity of the conclusions to the assumptions made going into the study.

Efforts also are made in both studies to decompose the innovation process and impose a fairly linear sequence of events on it. Again, trying to impose a linear sequence of discrete research events on what is necessarily a very complex and interactive process in which applied research affects basic, basic research in turn affects development, and the stages—the phases of the innovation process really interact; the attempt to impose a retrospective linear sequence on this process, I think, is very unrealistic and is not necessarily going to yield us very useful conclusions for policymaking.

The differences between the "Hindsight" and "Traces" studies do, however, illustrate the length of the time period that is typically necessary to reap the returns from basic research.

Mr. Chairman, I would then summarize my discussion by simply reiterating my general agreement with the conclusions of the OTA report and suggest that I recommend a strategy, as it were, that combines both some changes in the process of administration in the Federal Government with respect to basic research, as well as the application of some quantitative indicators at a specific level within the administration process on basic research, and I thank you for your attention.

[The prepared statement of Dr. Mowery follows:]

Policy Analysis and Federal R&D Funding Decisions: Are Quantitative Techniques Appropriate?

Testimony before the Science Policy Task Force, House

Committee on Science and Technology, April 29, 1986

David C. Mowery[1]

I am pleased to have the opportunity to testify before this Task Force on the utility of quantitative techniques for Federal basic research investment decisions. In the current environment of budgetary stringency, improved techniques for the allocation of Federal basic research funding are critical. However, existing quantitative techniques are irrelevant for the majority of the research funding decisions made by the Congress or high-level Executive branch policymakers. Basic research, which is the phase of the research process in the civilian sector that is typically supported by Federal funds, is characterized by high levels of uncertainty concerning outcomes. This characteristic of basic research reduces the applicability of quantitative techniques to basic research investment decisions. Moreover, alternative analytic techniques, such as bibliometrics, are incapable of dealing with the allocation of resources among basic research disciplines, a task that is central to Congressional decisionmaking on basic research. In general, therefore, I agree with the conclusions of the report of the Office of Technology Assessment on "Research Funding as an Investment: Can We Measure the Returns?" concerning the limited relevance of quantitative analytic techniques for these decisions. While available techniques are of little use, however, great benefits could be reaped from restructuring the decisionmaking process for basic research investment within both the Executive branch and Congress.

The fact that private industrial research laboratories rarely, if ever, make extensive use of quantitative techniques in evaluating and planning basic research investments should give pause to advocates of these techniques for Federal science policymaking. However, several aspects of the organization and management of industrial research carry with them important implications for Federal decisionmaking on basic research. These aspects include the linkages between basic and applied research that exist within industrial research laboratories, as well as the comprehensive character of industrial decisionmaking for basic research--competition for research funds among different scientific disciplines or areas of inquiry is enforced within the investment decision process. Neither of these principles is well-developed within Federal research policymaking. In the absence of a resource allocation process that allows for review and explicit consideration of the cross-disciplinary tradeoffs that are central to the operation of private industrial research laboratories, quantitative analysis is unlikely to improve the quality of Federal decisions on basic research investment. Moreover, the decisionmaking procedures embodied in the Gramm-Rudman-Hollings bill appear, if anything, to reduce further the possibilities for a comprehensive review of the Federal basic research funding in different disciplines or areas of research.

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[1] Associate Professor of Economics and Social Sciences, Carnegie-Mellon University, and Research Fellow, Center for Economic Policy Research, Stanford University. On May 1, 1986, Dr.Mowery begins serving as the Study Director for the National Academy of Sciences/National Academy of Engineering Panel on Technology and Employment.

My testimony is organized around a series of issues concerned with the characteristics of basic research and the processes by which research policy decisions are made within the Federal and private industrial research spheres. I first consider the appropriateness of viewing Federal R&D expenditures as investments, and go on to discuss different categories of R&D investment decisions made within the Executive branch and Congress. The next section examines the "lessons" from industrial laboratory management for Federal R&D decisionmaking processes. At the request of the Task Force staff, I have incorporated within my written testimony a brief evaluation of two well-known studies of research and innovation, HINDSIGHT and TRACES. Do these studies yield guidelines for research decisionmaking? Do they contain useful analytic techniques for Federal resource allocation decisions in basic research?

0.1 Does Federal R&D Funding Represent an Investment?

Investment expenditures in the standard economic definition are expenditures that yield benefits (defined as a larger bundle of goods for consumption) in the future. Defined in this admittedly abstract fashion, investment expenditures include a wide range of activities, including expenditures by individuals on schooling (expenditures that, by teaching these individuals skills, may allow them to command a higher wage in future employment) or expenditures by the Federal government on infrastructure, such as inland waterways. Expenditures by private industrial concerns on machinery also represent disbursements of funds that yield a larger future bundle of goods, due to the ability of such expenditures to increase total output.

The expenditures of private firms and the Federal government on the production of knowledge surely must be counted as investments. While the research process supported by these funds rarely produces a tangible commodity, such as a new waterway or a piece of machinery, the resulting knowledge undoubtedly enables firms to produce a larger bundle of goods. The OTA study concludes from an extensive survey of empirical studies that private firms and industries that invest more in R&D, especially own-financed R&D, exhibit higher levels of productivity and growth. The "knowledge capital" resulting from R&D expenditures constitutes an increasingly important input to the production process in both the manufacturing and service sectors of the U.S. economy. Indeed, the international competitiveness of U.S. manufacturing firms more often than not now stems from their endowments of knowledge, rather than physical capital.

The lack of a good theoretical model has frustrated the attempts of economists to trace the channels through which R&D expenditures enhance productivity growth and national income. While privately financed R&D expenditures appear to have a considerable impact on productivity, the evidence with respect to publicly financed R&D investment in industry is less clear. It is important to understand, however, that the tests applied by economists to determine the productivity impacts of Federal R&D investment in industry are imperfect--these studies do not allow one to predict the levels of productivity growth that would result in the absence of this Federal R&D investment. In this area, I feel that the conclusions of the OTA study must be interpreted with considerable care. The evidence supporting the conclusion that Federal R&D expenditures do not affect industrial productivity growth is weaker than the study implies.

There are at least two possible explanations for the modest statistical impact on productivity growth of Federally support R&D performed within industry. On average, 60-70% of the Federal funds expended on R&D within industry, a share that is much larger in some industries, is devoted to defense-related research. Defense-related expenditures are devoted largely to development, rather than basic research, and thus may yield results that require additional expenditures by private firms for application to commercial .markets. Military-civilian technological "spillovers," in other words, create technological opportunities that require the expenditure of private R&D funds. Federal R&D funds thus may induce higher levels of privately financed industrial R&D investment.[2] Despite the important role of Federal research funding in enhancing technological opportunities for recipient firms, however, statistical evidence of the impact of this Federal R&D investment on productivity still may be lacking, due to the fact that the R&D investments made by firms to exploit these opportunities pick up the statistical impact of the Federal research investment.[3] The withdrawal of Federal R&D investment from other industries might well exert a significant negative influence on the productivity impact of private R&D investments.

A second reason for the apparently modest impact of Federal R&D expenditures on productivity advance focuses on the differences between the productivity impact of Federally supported research in agriculture and the impact of Federal R&D investment in other industries. The evidence summarized in the OTA report suggests that Federally funded research in agriculture has a great impact on productivity. In contrast to most other Federal R&D programs, agricultural research incorporates a substantial investment in programs supporting the diffusion and adoption of research results. To the extent that the costs of gaining access to the results of publicly financed agricultural research are reduced by the USDA investment in the diffusion of these results, the productivity impact of the public research investment may be enhanced. In the absence of the supporting investment in diffusion, however, the impact on productivity of agricultural research programs might be no greater than that of Federal R&D in other industries.

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2. Empirical studies suggest that Federal R&D investment increases private R&D expenditures within industry, although Lichtenberg has questioned this conclusion. See Frank Lichtenberg, "Assessing the Impact of Federal Industrial R&D Expenditures on Private R&D Activity," presented at the National Academy of Sciences workshop on The Federal Role in Research and Development, Washington, D.C., November 1986.

3. See Nestor Terleckyj, "The Time Pattern of the Effects of Industrial R&D on Productivity Growth," presented at a Conference on Interindustry Differences in Productivity Growth, American Enterprise Institute, Washington, D.C., October 1984.

0.2 A Typology of R&D Decisionmaking within the Executive Branch and Congress

Several types of decisions concerning Federal R&D investment must be distinguished in order to clarify the areas in which quantitative analysis is relevant for decisionmaking. At the most basic level, policymakers consider the magnitude of the total Federal investment in basic research within all scientific disciplines and from all sources, including health and biological sciences, defense-related research, energy and physics. Whether explicit or not, a tradeoff is made every year between the shares of the total Federal budget allocated to basic research and the shares devoted to other ends, such as income maintenance. As several participants pointed out in the National Academy of Sciences workshop on Federal R&D investment, this decision ultimately is political. The term "political" is not used pejoratively; there exists no objective or quantitative calculus that enables one to compare the social benefits derived from an additional dollar devoted to research with those provided by an additional dollar devoted to income maintenance or national defense. The nature of the decisionmaking process within both the Executive branch and Congress is such that resource allocation decisions are rarely, if ever, posed in such stark terms. Nonetheless, this level is the most fundamental one for R&D decisionmaking, and it is one about which few but philosophers and politicians can speak with authority.

A second level on which R&D funding decisions are made by senior officials within the Executive branch and Congress concerns the shares of the total R&D budget devoted to one or another scientific discipline or field of basic research. Shifts in the allocation of basic research investments across scientific disciplines almost certainly affect income and productivity growth, although our knowledge of the channels through which this influence operates is modest at best. Currently, however, the decision processes within both the Executive branch and Congress do not support the comprehensive, cross-disciplinary review of all Federal basic research programs that is necessary to support such decisions; I return to this point below.

Can quantitative techniques be employed effectively for the allocation of basic research investments across scientific disciplines or research areas? The OTA report suggests that these techniques, including bibliometric analysis, are insufficient to this task. I support this conclusion. A central difficulty in the application of such techniques to decisions on basic research is the fact that economically meaningful measures of the outputs of basic research projects do not exist; we have only second-order, retrospective measures of impact, such as productivity growth. A second problem in applying these techniques to basic research decisionmaking stems from the distinction between risk, where probabilities can be assigned to each of a number of possible outcomes, and uncertainty, where the very nature of outcomes is unknown. As one moves from applied research and development toward basic research, risk declines and uncertainty increases in importance. Inasmuch as most Federal funding of civilian research supports basic research, analytic techniques employed in such other areas as finance or minerals exploration for the analysis and reduction of investment risk are of limited relevance for Federal decisionmaking on basic research, where uncertainty is central. Nonetheless, decisions concerning basic research are made within industrial research laboratories without resort to quantitative analysis,[4] utilizing an organizational structure and decisionmaking process that differ considerably from those of the Executive branch and Congress. Changes in the process by which these decisions are made within both the Executive branch and Congress that draw on the management practices employed within successful industrial laboratories would do much more than the application of quantitative analysis to improve the quality of Federal decisions on basic research.

A final level of research decisionmaking concerns the allocation of funds among different research teams or laboratories within a single discipline. The welter of detail associated with decisions at this level means that Congressional decisionmakers are well-advised to avoid consistent intervention in such matters. Nonetheless, the large capital expenditure programs for research facilities that occasionally are considered by Congress inevitably favor one research facility over another, and require comparisons of the performance of different research facilities within the U.S., or comparisons of U.S. and foreign research efforts. Bibliometric or patent-based measures of research performance are appropriate for research decisionmaking at this level of detail, and should be employed more extensively in oversight hearings and other appropriate venues within Congress and the Executive branch in assessing the performance of research facilities within a single discipline or research program. This level of the decisionmaking process, however, remains one in which Congressional involvement is unlikely to be extensive--agency personnel are better suited to make such decisions on a daily basis. The quality of the "mid-level" decisions, however, which affect the allocation of research funds across different disciplines, could be improved significantly through changes in the Congressional and Executive decisionmaking processes. The performance of private industrial laboratories provides much useful information, summarized in the OTA report, for evaluating changes in these processes.

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[4] Indeed, the OTA report notes that a number of management scholars recently have criticized U.S. corporations severely for excessive reliance on quantitative financial techniques for decisions on investments in physical capital and R&D. See W.J. Abernathy and R.H. Hayes, "Managing Our Way to Economic Decline," Harvard Business Review, 1980, and R.H. Hayes and D.A. Garvin, "Managing As If Tomorrow Mattered," Harvard Business Review, 1982.

0.3 Lessons From Private R&D Decisionmaking

The decisions of industrial research managers on basic research funding do not rely heavily on quantitative techniques. However, this reliance on quantitative techniques varies somewhat among the three levels of decision discussed above. The first two levels, concerning total corporate R&D spending on basic research and the allocation of this budget across research disciplines, exhibit little or no reliance on quantitative analysis. Evaluations of the performance of individuals or research laboratories, however, do occasionally employ quantitative measures, such as patents or publications. Nonetheless, the research investment decisions within the private sector that most closely resemble those made within Congress on R&D programs do not rely heavily on quantitative analysis.

The structure of the decisionmaking process within private research facilities, as well as the organizational structure of these laboratories, rather than their techniques for the evaluation of research funding decisions, contain important implications for Federal R&D decisionmaking. The two features of private research management and organization that are relevant in this regard are the enforcement of competition among different disciplines for the basic research budget within industrial laboratories, and the attention devoted by the management of successful private industrial laboratories to the establishment and support of linkages between the basic and applied research activities of the firm.

The ability of management within industrial basic research laboratories to review comprehensively the commitment of corporate funds across a wide range of basic research disciplines supports the application of informed judgment, in many cases reinforced by that of external academic reviewers, to research priorities. While quantitative analytic techniques typically are not employed in this allocation exercise, the ability to compare the likely benefits arising from basic research investments in one field, as opposed to another, is extremely important to the management of the R&D process. Within the Federal government, no comparable point of review of the full range of research investments exists. The Office of Science and Technology Policy has conducted occasional comprehensive reviews, frequently relying on the National Academy of Sciences' Committee on Science, Engineering, and Public Policy, but these are _ad hoc_ in character, rarely include defense-related basic research, and are not tightly linked with the resource allocation process. The Office of Management and Budget also conducts a review of civil R&D investments, but this review is not linked with high-level decisions on resource allocation priorities. The Executive branch review process needs to be strengthened, made more comprehensive (including its extension to cover defense-related basic research), and altered to involve senior policymakers throughout the Executive branch.

Fragmentation of decisions with respect to the Federal basic research investment, however, is even more severe on Capitol Hill. Civil basic research investments are considered by multiple authorizing and appropriations committees and subcommittees--there exists no point at which the full spectrum of disciplines and programs of basic research supported by the Federal government can be reviewed. While the research briefings of the National Academy of Sciences increasingly involve Congressional staff, the fragmentation of the process through which these briefings are translated into policy greatly reduces their impact. Decisions on the relative importance of different basic research disciplines and programs are made, but they are made in a fragmented, incremental fashion. In the absence of any Congressional capacity to review basic research investments in a comprehensive fashion, the priorities of the Executive branch will prevail. The recent adoption of the Gramm-Rudman-Hollings bill, with its provisions for equal percentage cuts in individual programs, exacerbates this fragmentation and may prevent any comprehensive review of the full span of basic research priorities within the Federal government.

How might the Congressional process of review and decision with respect to basic research investments be strengthened? Reorganization of the Congressional committee structure is unlikely. However, a select committee might be established, including among its members senior members of the authorizing committees charged with oversight of the numerous programs of civilian and defense-related basic research. Such a committee could, among other things, serve as a clearinghouse for briefings from outside experts and act to educate committee members and staff on informed experts' judgments of the likely payoff from basic research investments in different disciplines. Alternatives to a select committee include the establishment of a subcommittee of the House and Senate Budget Committees, charged with review of expenditures on all basic research programs funded by the Federal government. This option clearly is preferable to the select committee, due to the greater power that such a subcommittee could exercise over decisions on basic research investment. The specific mechanism chosen to support this comprehensive, cross-disciplinary review, however, is less important than is a recognition of the principles underlying this recommendation. In the absence of a restructured process for considering the investment of Federal funds in basic research, quantitative techniques of analysis are of little use.

The importance of linkages within private research laboratories between basic and applied research activities raises another issue of relevance to Federal decisionmaking on basic research. These linkages among the various stages of the research process, as well as those between research and other activities within the firm, increase the return to the firm's basic research investment. The higher returns associated with the development and sustenance of links among the phases of the research and innovation process reflects the fact that this process is not a linear, but an interactive, one--innovations do not "flow" from basic research in a unidirectional fashion. Instead, applied and development problems often exert a powerful influence on the direction and fruitfulness of basic research activities, as Nathan Rosenberg has noted.[5]

What implications does this interactive character of the research process contain for the organization of Federal research programs? Rather than rigidly adhering to a sharp division between basic research, where Federal financial support is viewed by policymakers as appropriate, and applied research, where such support is deemed inappropriate,[6] the establishment of linkages among the organizations involved in the different phases of the research process of the sort that improve the economic payoff from basic research investment may be aided greatly by Federal financial support, possibly on a sliding match basis. Many of the Federal research organizations displaying the most impressive returns on public investments in basic research, after all, have blurred the boundaries between basic and applied research. The National Aeronautics and Space Administration, for example, has long supported so-called "grey area" research in aeronautics, with considerable success. The programs of the USDA also extend well beyond basic research alone, incorporating active support of development, diffusion, and adoption. This model for Federal research investment may well merit extension to other sectors of the civilian economy.

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[5] See "How Exogenous is Science?" in N. Rosenberg, Inside the Black Box, Cambridge University Press, 1982.

[6] A clear statement of this position is found in Special Analysis K of the fiscal 1983 and subsequent Presidential budget documents.

D.4 HINDSIGHT and TRACES

I turn now to a brief consideration of the relevance to this Task Force's inquiry of the methods and conclusions of two well-known studies of the research and innovation process, HINDSIGHT and TRACES.[7] Specifically, do these studies provide conclusions or techniques that can support improvements in Federal decisionmaking for basic research investment? I think that the answer is no. While these studies are valuable descriptions of the innovation process, they are little more than that, due to the limitations of their methodology and subject matter.

The HINDSIGHT study examined the development of 20 weapons systems, quantifying their benefits (typically measured as improved cost effectiveness) and attempting to ascertain the importance of basic research, as opposed to development, activit'es in the development of each system. The study concluded that of the 710 "research events" deemed essential to the development of these systems, only 9% could be categorized as "science events," while 91% were "technology events." Significantly, despite the study's efforts to measure the benefits of improved weapons systems, attempts to compute a measure of the returns on the research investment of the Department of Defense were unsuccessful. The study in the end rejected the "...possibility that any simple or linear relationship exists between cost of research and value received."[8]

Does the methodology of HINDSIGHT hold promise for application to basic research decisionmaking within the Federal government? I believe that it does not, for several reasons. The study was unable to achieve a central objective, the computation of a rate of return on the investments in the research that resulted in advanced weapons systems. Any computations of the return on the investment, for which heroic assumptions would have been required, would at best be a retrospective, average calculation. Similar calculations, even if they were feasible, for the civilian basic research investments of Federal agencies would tell one virtually nothing about the _marginal_ benefits to be gained _in the future_ from allocating additional funds to one or another field of basic research. Moreover, the structure of the HINDSIGHT study resulted in a considerable bias against the ascription of importance to basic research discoveries. The arbitrary 20-year cutoff for research events assumed away the significance of scientific and basic research discoveries for weapons systems development. The study also did not attempt to distinguish among the importance of individual research events, further reducing the utility of its calculations as a guide to the allocation of research funds across scientific disciplines. The equal weighting of all discoveries contributes to additional undervaluation of the contribution of basic research, as the study itself notes: "...wherever there was a payoff of basic scientific research in terms of use in systems, its value was comparatively great."[9]

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[7] See Project HINDSIGHT: Final Report, Office of the Director of Defense Research and Engineering, 1969; Technology in Retrospect and Critical Events in Science, National Science Foundation, 1969.

[8] Project HINDSIGHT: Final Report, p. xxii.

The HINDSIGHT study examined a unique innovation process, with few direct analogues in the civilian economy or elsewhere in the Federal research budget. The research programs of the Department of Defense, after all, extend well beyond basic research. Moreover, the computation of a notional rate of return to these research investments is considerably less complex when the products embodying this investment are introduced into a nonmarket environment. The equation of the performance and behavior of the market for defense weapons systems with the market for civilian innovations is a very questionable logical leap, yet necessary to apply the "lessons" of HINDSIGHT to other areas of basic research decisionmaking. Two other conceptual flaws in the research design underlying HINDSIGHT should be noted. The study adopts an unrealistic "linear" conceptualization of the research process, in which applied research events draw on basic research, development in turn draws on applied research, etc. In fact, the innovation process appears to involve a great deal more interaction among the various stages and types of research than this conceptualization suggests. The model of the innovation process embodied in HINDSIGHT also views basic and applied research activities as somehow competitive, rather than complementary, in supporting innovation. Rather than interacting with and supporting one another, basic and applied research in the HINDSIGHT study are competitors. Such a conceptualization may be true for a fixed research budget allocated among these two activities, but has little to do with the actual behavior of the innovation process.

Partly as a response to the conclusions of HINDSIGHT that basic research was of little significance within the development of weapons systems, the National Science Foundation commissioned the TRACES (Technology in Retrospect and Critical Events in Science) study, which abandoned the 20-year limitation in retrospectively determining the role of research events in the eventual appearance of five significant innovations. In addition to its lengthier time horizon, TRACES also changed the focus of study somewhat, in examining major innovations produced for the commercial market. TRACES made no attempt to assess the returns to the investment in the research events resulting in these innovations.

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[9] _Project HINDSIGHT: Final Report_, p. 96.

The findings of TRACES are scarcely surprising, given the changes in methodology--basic research (referred to as "nonmission-oriented research") played a major role. The study concluded that some 70% of the important research events in these innovations resulted from nonmission-oriented research, while 20% of the events were mission-oriented. Moreover, 45% of the nonmission research events deemed important by TRACES occurred more than 30 years prior to the appearance of the innovation, demonstrating the sensitivity of these studies' conclusions to the time horizon adopted by each. While TRACES continued the attempt to identify discrete "research events," its conceptualization of the research process acknowledges more explicitly the interaction of mission- and nonmission-oriented research activities and outcomes in the innovation process.

While both of these studies contributed to a broader understanding of the complexities of the innovation process, their "lessons" for research investment decisions within the Federal government are very limited indeed. Their focus is exclusively retrospective, rather than predictive. HINDSIGHT was unable to provide estimates of even the average returns on past investments in research, despite its resort to arbitrary assumptions concerning the time horizon within which research affects innovation. Finally, the attempt by both studies to identify and assess the contribution of isolated "research events" to innovation requires arbitrary assumptions and an unrealistic view of the innovation process, which is both more interactive and frequently more continuous than is implied by the efforts of these studies to isolate and quantify the contribution to innovation of individual research events.

0.5 Conclusions

I support the conclusions of the OTA report on Federal investment in research, which suggest that quantitative analytic techniques are of questionable value to Congressional and high-level Executive branch decisionmaking for basic research. There is an important role for bibliometric and other measures of research performance in more detailed comparisons of research teams and laboratories within a single scientific discipline, however, and measures to increase the use of such measures within Congressional and Executive branch oversight and evaluation would be useful. Nonetheless, for much of the decisionmaking process in the Executive branch and Congress, the most important policy implications of an examination of the management and organizational practices of successful industrial laboratories concern the reorganization of the process through which decisions are made on basic research investment priorities. Both the Executive branch and Congress, but especially the Congress, need to develop decisionmaking procedures that support a comprehensive review of the importance and likely contributions of additional investment in basic research indifferent scientific disciplines, relying heavily on the informed judgment of persons outside and within the Federal government. Scientific disciplines and research programs must compete for funds within a unified basic research budget, rather than being evaluated incrementally and separately by a maze of authorizing committees and appropriations subcommittees. In the absence of a more powerful Congressional decision apparatus for the establishment of basic research priorities, decisions remain difficult to defend, and Congressional decisionmakers are largely forced to accept the judgments of the Executive branch. Finally, the Gramm-Rudman-Hollings procedure operates to entrench incrementalism in reverse, preventing precisely the comprehensive, cross-disciplinary review that is badly needed for better informed decisions on basic research investment.

DISCUSSION

Mr. **Fuqua**. Thank you very much, Professor Mowery.

You're talking about the comprehensive review by the executive branch and Congress, and you say in the conclusion that that's necessary.

Dr. **Mowery**. Yes.

Mr. **Fuqua**. What are we supposed to review for? And not to try to answer my own question or put words in your mouth, but are we looking at long-range—how do we—if you get a review, and you don't know any more at the end of it than you did at the beginning, how then do you make intelligent decisions that these are areas that we need defined? It gets back to the issue that I was talking about earlier—you were here—about accountability.

Dr. **Mowery**. I would suggest that a review of this sort would involve the application of the expert judgment of individuals both within and outside of the Federal Government. This is essentially what industrial research establishments do.

There are no right answers, there are no perfect predictions, but my point really is that this is the only useful input one can get for making these basic research decisions. We don't have useful quantitative predictors, and the point is really to try to set up a venue within which this expert judgment can be applied, across the board, as it were, to the basic research investment of the Federal Government.

I would suggest that the application or the establishment of such a procedure potentially could identify fields that contain significant opportunities that currently are not receiving substantial funding, and, conversely, identify fields that are perhaps too richly funded within the basic research programs of the Federal Government. I think the accountability could be established, Mr. Chairman, by virtue of the fact that a broader outreach would have been clearly perceived and established as a direct input into this administration process, and that's the sense in which I think the hearings of something like this task force or a select committee would serve very effectively to educate Members and staff as to who sees and what they see as valuable basic research areas. It's a process, though, it's not a magic answer.

Mr. **Fuqua**. Another problem develops in where you put your priorities. If you talk to the physicists, they're the most important; if you're an economist, you're the one that understands how all this works; and if you're a mathematician, you can't do economics without understanding math; and if you're an engineer, you're the one that put it together and makes it work in the applied area that you get a return on investment, and blah, blah, blah.

Now I think what we're talking about is what kind of indicators and what level do you feel that need to be developed that can assist policymakers in trying to make intelligent decisions, and then within that you have those that may have their own agendas or priorities that may be right, or they may be off base. But what kind of indicators can we develop through some type of methods to try to give us some intelligent information to make decisions on?

Dr. **Mowery**. I really don't think that we are likely to arrive at indicators that allow us to say the return on investments, if we allocate an extra $100 million to high-energy physics, is going to be 12 percent, and the return on investment of an additional $100 million in agricultural research is likely to be 9 percent; I really don't think that's likely to happen.

I think you have to go with the qualitative estimates of experts, I imagine that these briefings would incorporate some speculative—admittedly speculative—discussion of likely impacts of research breakthroughs in these areas of basic research, but I don't see it. I'm an economist; I don't see economists developing these strong predictive techniques for computing the return on investment, and I would suggest that if you find an economist who is doing this——

Mr. **Fuqua**. Grab him.

Dr. **Mowery**. Well, question his advice, or he's going to sell you a bridge next.

Mr. **Fuqua**. I know. That's what the late President Truman always said: he wanted a one-armed economist.

Dr. **Mowery**. Yes. Well, I'm afraid I have two hands, unfortunately.

Mr. **Fuqua**. But on the other hand, are there any unique techniques that industry uses? We can make a mistake in public policy and probably get by with it, but in industry you may, if you invest a lot of money in R&D—of course, it's probably more mission-oriented or market-oriented in your particular field, but if it fails, you really not only waste a lot of time and effort but maybe have set your company back a number of years because of the wrong approach that you took. Are there any secrets that they have that we don't have in the public forum?

Dr. **Mowery**. I think that the OTA report and the individuals who put together the OTA report did an excellent, and a very thorough, job of beating the bushes for secrets in industry, and there really are none.

I think one thing that is important to keep in mind in thinking about industry, particularly its funding of basic research, is—I'm obviously not a research manager; I am a close observer, and only an observer, I'm not a participant—but the goals of basic research in many industrial research laboratories are not solely to increase profits, or they are not directed to. The investment in basic research is viewed as yielding a range of intermediate outputs, if you will, that contribute to higher profits.

A large basic research program allows you to get better staff who can contribute significantly to applied research projects, but you have to really take into account the complexity of the goals which even industrial research managers employ in funding basic research. They are not looking solely at a return on investment for the basic research dollars they invest; they are looking at the whole complex of outputs from basic research, which include better staff, which include the ability to contribute to academic proceedings and meetings. Not all of this translates in the long run into higher profit, but in the short term and in the quantitative techniques they don't employ, it does not directly show up in a return on investment. It's very important, to keep in mind that there are a number of objectives pursued by private sector, industrial, basic research investments, just as there are in the public sector.

Mr. FUQUA. But thus far the accountability has been that, yes, we have been able to get a handle on polio and other forms of medical research that we have made significant process. We see the evening news, and satellite transmissions of weather, and pictures, and so forth, and communications. So we have a perception that something is working right that we can see, but there's still no indicator that says if you spend $1 here today that you get something back in 4, 5, 7 years in return on your investment.

Dr. MOWERY. Not in basic research. I suggest that's correct.

Mr. FUQUA. Should we—should Congress broaden its view on research funding to include more emphasis on applied research rather than basic in order to try to provide a bridge between those two, and then it would give us maybe a better knowledge about what we're doing?

Dr. MOWERY. This relates to a question that you raised earlier in terms of looking at the returns to public Federal investment in industrial R&D, the productivity impact. I discuss this a bit in my written statement.

Two things one has to keep in mind are that a great deal of the Federal research investment within industry is defense related, and therefore within that defense-related component—I'm talking now between 60 and 70 percent of the Federal investment in research performed by industry—within that component, a great deal of this is development work, not basic research. Therefore, you face a situation in which a great deal of the Federal moneys invested in research performed by industry may create technological opportunities, but the exploitation of these opportunities, and their productivity enhancing effect requires the investment by firms of their own funds, and therefore, to some extent, the tests employed by economists in looking for a productivity impact of federally funded research within industry are weak. They do not really address the question of what productivity would look like if we didn't have any Federal funding of research within industry. I think the conclusions are somewhat weaker than is suggested by some of the analyses I've seen.

The other point I would make about the payoff to federally funded research performed within industry is that federally funded research in agriculture, where there seems to be quite a bit of agreement on the productivity impact, differs in structure. One of the things that's quite different is the research investment of the Federal Government in agriculture includes, or is complemented by, a substantial investment in diffusion activities supporting the transfer of the technology to the private sector.

Historically within the Federal establishment we have focused very much, with the exception of agriculture, on generating rather than worrying about the transfer of technology, so I would suggest that some consideration of activities designed to support the transfer of technologies is appropriate, and this might also include blurring what I think is an excessively rigid boundary set forth in special analysis K and elsewhere between basic research, where Federal support is appropriate, and applied research, where Federal support is inappropriate. That distinction itself really employs a linear model, if you will. Basic research is the wellspring from which all innovation flows rather than basic research interacting with applied research, and that really is a second lesson from industrial research establishments. The successful industrial research establishments work very hard to retain close links between applied and basic research to retain the interaction between the applied and basic research phases of the innovation process, and this interaction is something that is really tossed out by a mandate that—a sort of self-denying ordinance—that prohibits Federal funding of any research beyond the most basic phase or process.

Mr. FUQUA. Mr. Slaughter.

Mr. SLAUGHTER. No questions.

Mr. FUQUA. I guess one of the things, too, that we face is that much—not much—some of the basic research may sit on the shelf for 30 years before some researcher is scouring the library for data and runs across something that has an application.

Dr. MOWERY. That's well brought out in the "Traces" study.

Mr. FUQUA. And there's also the fact that a lot of the basic research, probably most of it, is carried out in our colleges and universities, and one of the primary missions of those is to train future scientists, and that, I think, we sometimes tend to forget, that that's the training ground.

Dr. MOWERY. Exactly.

Mr. FUQUA. And without that we lose all of that, and it would be the equivalent to the cultural revolution that they had in China.

Dr. MOWERY. I think that's exactly right, and we see a substantial increase in industry funding of research within universities, but we're talking about an increase from a very small fraction of research within universities to perhaps a slightly larger fraction, from perhaps 5 to 7 percent, maybe 9 percent at the outside, by the 1990's. Industry research support is not likely to replace Federal research support for university research and training activities. Industry is not capable of making up the cutbacks in Federal research.

Mr. FUQUA. No.

Thank you very much.

Dr. MOWERY. Thank you.

[Questions and answers for the record follow:]

Responses to Followup Questions:

David C. Mowery,
Director, Panel on Technology and Employment,
National Academy of Sciences

1. Would you advocate the revision or rescission of restrictions on funding for applied research that you cited in Special Analysis K?

The arguments underlying the analysis of Special Analysis K of the annual Presidential budget document employ an unrealistic conceptualization of the process of innovation, and overlook the experiences of successful Federal programs of assistance to commercial technological development and innovation. Innovation does not proceed in a "linear" fashion, in which basic scientific discoveries are developed through separate, applied research and development activities. Instead, the innovation process is a nonlinear, iterative sequence, in which applied research and development findings and results operate to inform and mold the basic research agenda, and **vice versa**. Successful industrial research laboratories work very hard to preserve the links between basic and applied research activities, reflecting the recognition by private research managers of the importance of such links. Moreover, the most successful Federal programs of support for innovation in the private sector, a group that includes Federal agricultural research and the aeronautical research programs of the National Advisory Committee on Aeronautics and the National Aeronautics and Space Administration, also have avoided a sharp division between basic and applied research activities in their research support. In view of the fact that the Office of Science and Technology Policy endorsed NASA funding for applied (so-called "grey area") aeronautics research in its 1982 report, Special Analysis K seems inconsistent with other science and technology policies of this Administration. I therefore would advocate the selective revision of the prohibition on funding of applied research that is embodied in Special Analysis K.

2. In your concluding remarks, both oral and written, you recommended changes in the process of administration "--with respect to basic research as well as the application of some quantitative indicators..." and that "...The Congress needs to develop decision-making procedures that support a comprehensive review of the importance and likely contributions of additional investment in basic research..." Please be more specific defining steps the Congress might take to achieve these goals.

This question addresses the fundamental message of both my testimony and the Office of Technology Assessment report. The specific quantitative techniques employed to evaluate the payoff from research investment are much less important than is the venue within which they are employed. Within both the Executive branch and Congress, but especially within the latter branch, decisions on funding levels for basic research programs are made in a fragmented, piecemeal fashion. The current process, in which no less than four authorizing committees and a like number of appropriations subcommittees separately determine funding for basic research programs in different areas, is not conducive to the sort of comprehensive, cross-disciplinary review of basic research priorities and payoffs that is so important to the development of better informed, more defensible resource allocation decisions in this important area. While I agree with the OTA report's conclusions that any such process of comparison and comprehensive review cannot employ quantitative analytic techniques (they do not exist or are irrelevant), qualitative, informed judgments can be made by the Congress, relying on the expert advice of such groups as the National Academy of Sciences or the National Academy of Engineering.

How can the Congressional processes of program oversight and resource allocation be structured so as to allow for such a comprehensive review of research priorities? The simplest answer is a reorganization of the standing committee system, but this is unrealistic. Alternatives to this approach include:

> Formation of subcommittees of the House and Senate Budget Committees, charged with undertaking such a review in the course of the formulation of the annual budget resolution.
>
> Creation of select committees in the House and Senate for the evaluation of research funding priorities. These select committees would include the chairmen and senior members of the relevant authorizing and appropriations committees and subcommittees, and could serve an important educational and evaluative function, drawing extensively on the analyses of outside experts.

DCM
6/18/86

Mr. **FUQUA**. We'll now hear from Ms. Mary Tanner, managing director of Shearson Lehman Bros.

[A biographical sketch of Ms. Tanner follows:]

MARY C. TANNER

Ms. Tanner is a managing director in the Investment Banking Division of Shearson Lehman Brothers Inc., one of the largest investment banking and brokerage firms in the United States. At Shearson Lehman Brothers she has responsibility for managing a group of professionals who focus on the health care industry. Ms. Tanner joined Shearson Lehman's predecessor firm, Lehman Brothers, in 1977 and was elected a principal of that firm in 1983. Cited in Institutional Investor in 1984 as one of only 11 women who has achieved principal status in a major Wall Street firm, Ms. Tanner has particularly focused on financing high technology and health care enterprises. Prior to Lehman Brothers, Ms. Tanner worked at the Manufacturers Hanover Trust Co., and in a private venture capital partnership.

A 1973 honors graduate of Harvard University in philosophy and related fields, Ms. Tanner was raised in the New York area. She is a frequent speaker at private sector industry and trade functions for the biotechnology industry, which has been one of her particular areas of business interest over the last 10 years.

STATEMENT OF MARY C. TANNER, MANAGING DIRECTOR, SHEARSON LEHMAN BROS., INC., NEW YORK, NY

Ms. **TANNER**. Good morning, Mr. Chairman and members of the committee.

Mr. **FUQUA**. Good morning.

Ms. **TANNER**. And I would like to thank you for the opportunity to participate in this subject which is, frankly, of immense interest to the private sector and to Wall Street as well, and I would also like to echo the comments of some my colleagues——

Mr. **FUQUA**. Would you mind pulling that microphone just a little closer to you?

Ms. **TANNER**. Better?

Mr. **FUQUA**. Yes.

Ms. **TANNER**. I'd also like to compliment OTA on what I think is a very fair and quite comprehensive summary of a considerable amount of thinking that has been done on a complex problem. I would echo the fact that the private sector doesn't have answers for you, but I think there are some specific things we can say in response to questions you've asked other witnesses today.

Let me note that in order to define "return" in any program, whether it is in the public or private sector, it's prima facie necessary to have a clearly defined goal. It also helps to have a clearly defined timeframe. Given the size, scope, and organizational structure of the Federal research commitment as well as the enormous plurality of constituencies it serves, it is prima facie impossible to have a single overriding goal or therefore a single overriding way to assess return. So for my purposes this morning, I used a division that I think many people have spoken of over the years, which is to divide Federal research into three functional areas: aiding U.S. industry; substituting for investment by U.S. industry; and furthering broader specific social or rather types of public policy goals.

My comments today will focus exclusively on methods of improving Federal research where the first goal is the case, which is to aid or augment private sector research. Most of this research is, of course, in the applied sciences—health care, electronics, and so forth. My comments do not relate to what some folks call big science, and those are specifically kinds of disciplines where the amount of capital investment is so enormous that only Federal research can provide a meaningful contribution or base for pursuing those disciplines.

Now, definition of research goals in the private sector are a great deal easier than in the public sector because in the end, while I think, as a previous witness noted, there may be a number of interim steps, the ultimate goal is commercially usable inventions that can be successfully sold and produce a net increase in stockholder wealth, usually measured in company earnings, and stock price, and dividends. So first of all, the singularity of that goal gives private industry a relative advantage over you gentlemen here today.

Second of all, I'd like to remark that I think the way industry thinks about setting up its research priorities, the size of its research investment, and perpetuating that over time is very much the way they think about the general requirement they have for plant and equipment. For example, on Wall Street, if you are an astute analyst of drug firms—pharmaceutical companies, you will know that most pharmaceutical companies carry research and development percentages of sales from 9 to 12 percent consistently over a 20-year period, and in fact Wall Street will wake up and take notice if Merck suddenly is spending less than that a year. So in a sense, I would suggest to you that private industry regards its research and development budget, if you are a technologically oriented enterprise, as a sort of capital asset in a funny sense, and, if you will, analyzes it over time like its capital asset base, and I would suggest to you that there is some merit, although there's a lot of nonquantitative thinking involved in that conceptual leap, to your endeavor here today, and that is that, in fact, as a country the Federal research budget is a kind of asset of ours and could be thought of in the same way that a pharmaceutical enterprise might think of its budget over time.

The second thing I'd like to remark is that research budgets are set not dissimilarly in private industry from the way you set a budget here in Congress in consultation with the executive branch. There are certain financial goals which are relatively immutable if one's stock price is to stay where it is and one's shareholders are to prosper over time, and that means that research will get a certain cut of the available financial profile of the company, and there will be strict resource allocations down a scale from that. The way industry does it is on a basis of priorities; the largest amount of dollars get the most focus first, which makes a certain practical sense, and what that means in terms of research is that the chief executive officer and research director of a firm will first look at those projects that are nearest to commercialization, because those are the projects that involve the largest amount of dollars and therefore where the mistake can be the greatest and you have to fix it first. Generally, chief executive officers of companies will not be involved in specific project review of budgets below a certain dollar amount unless they happen to be an executive who came out of research in that firm and therefore have a particular personal interest in it, or if there are certain types of basic, and not large-dollar research going on that is expected to have enormous commercial impact on the company.

Now what does that mean? It means that basic research is managed at lower levels of the company and that senior executives use rules of thumb to see that everything's going fine and that the capital stock that I have referred to, if you will, of research in the company is well and thriving and is not being worn down.

For example, in pharmaceuticals, a chief executive officer would review how many clinically interesting compounds emerged from basic research in the course of a year. A typical drug company would screen somewhere between 2,000 and 5,000 a year, and you ought to have at least a 10-percent hit rate, if you will, out of that screening activity.

Each of those sorts of normative chief executive officer rules of thumb are different in different industries so it's not very easy to summarize them for you, but you look first at where the biggest mistake can be made and then work backward from there as time permits. And I would also like to generally comment that while there's a certain intellectual appeal in the notion that there is enormous competition for the marginal dollar in research in corporations reflecting the general competitive environment in which they operate, I think that is intellectually appealing but practically not true. In fact, the most successful research organizations over time—and I apologize to our previous witness—the most successful research enterprises over time, by my observation, are ones in which there is extraordinary competition and sharing between different groups of researchers in a company.

Also, another thing for you to remark on perhaps is that the most successful research enterprises in terms of Wall Street's view of them over time are people who have concentrated in bands of technology. For example, a corporation like Raychem Corp. in California may not be well known to you but is the leading supplier worldwide of irradiated plastics and cryogenic materials. They have been concentrating in those two technologies for 25 years, and therefore, if you will, their incremental gains can be very profound. The development of new industries on Wall Street like biotechnology, which will be my subject in some remarks later on, largely comes from existing competitors in an industry not noticing a new technology because it does not emerge from their incremental gains over their R&D capital stock base.

Now it is true, as OTA suggests, that the only readily used quantitative method of analysis in the private sector is generally referred to by a class of terms called capital pricing models—discounted cash-flow analysis—and they are generally applied to later stages of research. Despite the quantitative aura of these models, I would point out to you that the most important inputs are fundamentally qualitative and involve judgment by managers: assessment of the time and cost of research, the cost of manufacturing capacity, and of course, most importantly, commercial issues.

The OTA memorandum, I think, highlights in an extremely interesting way the same sorts of qualitative problems and qualitative inputs that create problems in dealing with, for example, quantitative econometric analysis if you're trying to study the impact of Federal research on the economy, and that memorandum points out on page 22 to 23 that a major stumbling block is that you're looking at input, which is investment, and output, which is productivity changes, and you're missing everything in between. In fact, it is those steps in between that are critical to turning laboratory research into commercial products, and OTA goes on to say in that memorandum: "However, the Federal Government has direct control only over its R&D investments, and very little influence over commercialization in the private sector." I would submit to you that, in fact, the most powerful influences on those intermediate steps from the research laboratory to commercialization are those indirectly exercised by the Federal Government through fiscal and monetary policy and their effect on capital markets, trade policy, tax incentives, patent law, and regulation, in that priority, as I have observed them in American industry over the last 13 years as a banker.

Let me just give you a few illustrations, and I think it's possible to put some numbers on those. So if the committee wouldn't mind, I'll just pass around some figures for you to look at.

Let me first talk about fiscal policy which will be useful, perhaps, for illustrating what discounted cash flow can show you.

Fiscal and monetary policy determine financial return on investment criteria for the private sector, and the reason is that all discounted cash flow models proceed off what is called the risk-free rate, and the risk-free rate is generally determined by the interest rate at which the Federal Government borrows on a 10-year or 30-year basis. The thesis of these models generally is that an investor facing a choice of investment alternatives will compare a risk-free investment like U.S. Government bonds to his alternatives. These examples are about biotechnology stocks, which are in fact among the riskiest of investments you can make, particularly in this equity market. The rational investor obviously will demand a higher return, and he will price that return according to his perception of risk.

In March 1985, I made a speech in front of a biotechnology conference in which I suggested to them that that premium was about three times the Federal 30-year bond rate, which was then 11.8, suggesting that the required rate of return for me to buy their stock was about 35 percent.

Now the situation, of course, is very different today. Treasury bonds are at about 7.5 percent. I think the risk premium is still about threefold, but that only turns out to be about 18 to 20 percent. So if I would ask you all to look at table 1 just briefly, and you will see the point, which is that the difference between, over the same time period, a 20 percent and a 35 percent required rate of return creates roughly a 30 percent difference in that stock price.

TABLE 1

ANALYSIS OF STOCKHOLDER'S RETURN ON INVESTMENT IN COMPANY A BIOTECHNOLOGY STOCK

REQUIRED RATE OF RETURN	35%	18-20%
STOCK PRICE IN 3 YEARS (TO CREATE REQUIRED RATE OF RETURN)	\$37	\$37
RESULTING STOCK PRICE TODAY	\$15	\$21-22
% DIFFERENCE		32%

Now obviously that has impacts on capital formation for the biotechnology industry, and therefore the first conclusion I would ask you to take from this table, simple as it may be, is that one of our greatest national assets is, in fact, our U.S. capital markets which offer much better opportunities for firms to raise equity for long-term investment than those of Europe of Japan. Substantial sums are available in this market to finance even the riskiest, early-stage research-oriented enterprises like biotechnology. But, when a very high degree of uncertainty exists regarding fundamental rates of return which are determined by fiscal and monetary policy and also obviously by inflation implicit as a result of those, higher risk investments, such as biotechnology, are crowded out of that market, and capital formation is indeed a very great challenge to commercializing basic research that may have been produced over the years by the Federal budget.

The second thing I'd point out is the consistency of pricing for capital is a very important thing for the public sector. For example, the Japanese are often touted for their ability to make long-term decisions. I would point out to you that a Japanese investor looking at that stock on his local market would have used the same rate of return in either May 1985 or May 1986, and a Japanese manager evaluating an investment in that same period would have had that advantage.

Let me, if I could, illustrate that to you by table 2, which will show you a stream of expenditures and profits from two new pharmaceutical compounds. You can see the difference at the bottom of the chart in the net present value, both at different required rates of return and in different time frames, which I'll come back to.

TABLE 2

THERAPEUTICS VALUATION

PROJECTIONS

($ MILLIONS)

	COMPANY		
	REVENUES	PRE-TAX	CASH FLOW
1986	--	$ (2.9)	$ (2.9)
1987	--	(6.1)	(6.1)
1988	--	(15.2)	(15.2)
1989	--	(20.5)	(20.5)
1990	--	(13.8)	(14.1)
1991	$ 6.1	(12.0)	(18.2)
1992	74.6	31.7	3.2
1993	207.5	126.4	28.7
1994	394.1	254.9	50.8
1995	589.1	376.2	109.1

RETURN ON INVESTMENT AT RATES OF: (A)

20%	$496.9 MILLION
35%	$148.9 MILLION

CASH INVESTED $77.0 MILLION

(A) INCLUDES RESIDUAL VALUE

A Japanese company would have been evaluating this investment at the same rate of return in the 2 years I've discussed. Are the Japanese more rational? I'd submit to you that if an American manager had that luxury, he'd be equally rational and make long-term investment decisions, too. So I'd like to point out—and OTA's study notes it—that in the 1950's and 1960's when we had a very stable monetary and fiscal policy, there was a very high level of research performed by American industry, and the OTA study says that the 1970's showed a significant slowing of this investment flow because industry became skeptical of demonstrable return.

Industry didn't become skeptical of demonstrable return on research in the 1970's and early 1980's; what happened is that their timeframe for investments was forced by economic and capital market conditions to become significantly shorter, and obviously, short timeframes mediate against stable long-term investment in research.

Some of the similar kinds of problems are forced upon you today, if you will, in the context of a budget crisis, but I think that it's important to remember that what goes on here in Washington has immense impact at the margin because for the riskiest of enterprises like technology companies, they can only thrive and receive private sector investment if the risk-free rate is low, meaning the basic economic and fiscal condition of the company is strong—of the country is strong.

The second thing I'd like to point out is that trade policy has a very important impact on commercial applied research productivity. This is a very complicated subject and one in which I am not expert, but let me just give you two vignettes.

The first is obviously the administration's persistent and, I think, very appropriate prosecution of antidumping procedures against Japanese semiconductor manufacturers. I'm not advocating erecting protectionist barriers, but I would like to point out to this committee that the largest dollars are spent in the very late phases of commercialization, and in order to get a return on that investment, you have to be able to go out and sell in the market and produce profits. I do think that the ruthless and energetic prosecution of our rights under established trade treaties is very important to American industry, and has been not effectively pursued by this administration until recently.

I'll mention one other example which is quite famous in the drug industry. The amount of time required by the Japanese patent office to approve a local patent on a U.S. pharmaceutical is dramatically longer than the reciprocal treatment a compound receives here. Now much of this information is actually anecdotal, but a considerable amount of it has been quantified. I think U.S. Government trade initiatives in this area could produce direct industrial results and is important because, in fact, a very large component of the Federal research budget is provided by NIH and NCI, and many pharmaceutical compounds emerge into commercialization from that research base. So I think a little focus on the treatment given patents by European as well as Japanese patent agencies would be useful.

Another thing I'd point out is tax policy. It's curious to me, given the scope of the Federal budget devoted to research and the incredible efficiency demonstrated over the last 30 or 40 years of tax incentives in directing investment flows in this country, that little focus has been given to the potential benefits of increasing private sector research, and therefore also increasing their application of certain basic research that may have become available because of Federal funding through tax policy.

The research and development tax credit has been a stepchild in this Congress for many years. As I understand, it is currently in force, but in its present form the credit sharply favors large, established industrial research enterprises. It, in fact, significantly disadvantages smaller industrial enterprises, which is, in fact, where some of the most innovative applied research has been accomplished in recent years. The biotechnology industry is a good example.

Internal Revenue Service treatment of R&D partnerships, the pass-through of excess R&D tax credits to individuals, a number of tax policy issues, I think, could be very fruitfully applied to increasing the rate of return on investment in this country, and while there is no direct linkage necessarily between the benefits that such tax policy would give private industrial companies and increasing Federal research returns, it seems intuitively clear that there are indirect ones.

Finally, patent law. First of all, I think industry gives the U.S. Patent Office in this country very high remarks in general for their thoroughness, their ability to rise to new technologies, and their willingness to really pay a lot of focus to what industry is saying. There are a couple of weak areas, such as, for example, software and semiconductors, a couple of issues not related exactly to the operation of the Patent Office but to patentability in general are very important to you.

Incontrovertible assignment of patents received under federally-funded research is enormously important to industrial enterprises in order to encourage them to invest the dollars that are needed for commercialization, which are significantly larger, obviously, than the dollars that were put into research. Weakening of those procedures to require broad-based compulsory licensing—which is a subject I know has been discussed in other areas of the Federal Government—under the aegis of "creating competition" will, in fact, only decrease U.S. taxpayers' research dollar return, because it will lower incentives for companies to commercialize the inventions. I think "competition" is a buzz word which is wise to think about in many contexts, but competition with respect to proprietary rights is a direct disincentive to commercialization.

And lastly regulation, another very complex subject. I would like to point out just a couple of vignettes, and that would cause you to look at table 3, if you would, for a minute.

TABLE 3

THERAPEUTICS VALUATION

PROJECTIONS

($ MILLIONS)

	REVENUES	PRE-TAX	CASH FLOW
1986	--	$ (2.9)	$ (2.9)
1987	--	(6.1)	(6.1)
1988	--	(15.2)	(15.2)
1989	--	(20.5)	(20.5)
1990	--	(13.8)	(14.1)
1991	$ 6.1	(12.7)	(18.9)
1992	19.5	(10.2)	(15.2)
1993	35.5	(13.4)	(22.1)
1994	94.8	11.9	(14.1)
1995	217.8	61.1	(0.4)

RETURN ON INVESTMENT AT RATES OF: (A)

20%	$103.4 MILLION
35%	19.3 MILLION

CASH INVESTED $129.6 MILLION

(A) INCLUDES RESIDUAL VALUE

Table 3 is the same data you saw on table 2; there's only one difference, which is that one of the two compounds that are shown in that cash flow series is introduced 2 years later; rather than in 1991, it's introduced in 1993. If you look at the bottom part of that chart, and you look at return on investment numbers and cash invested, you can see that a 2-year delay in this compound, which is a major compound, doubles the cash investment and reduces the net present value to investors by two-thirds. The numbers are roughly 150 to 500 million, depending upon what rate of return you require, and if there's a delay, $19 to $103 million.

Regulation is enormously important, and it is especially important in research enterprises where we're required to keep to very tight time schedules to be able to provide rates of return to their investors if those investors are private sector individuals. I think it is important for this committee to try to choose some selected legislative initiatives to deal with some of these barriers to commercialization of research, because there's no centralized committee in this Congress that vigorously defends U.S. technology in the way that there are significant congressional initiatives on defense, or intelligence, or other things of enormous importance to the United States.

The inchoate action of various things like fiscal and monetary policy, increasing regulation, lack of any tax incentives, influences that change rules on patent laws, can diminish return in the private sector to very low levels and unacceptable levels, such that you cannot stimulate investment by our very deep and rich capital markets to look at industries like biotechnology. I think that legislative initiatives to allow U.S. capital markets to do what they can do so well would be very well received by industry.

Most of my industrial clients, a number of whom I called before I came down here today, when I said to them, "Well, what would you like someone in Congress to hear?" they said, "We don't, in fact, really need a lot of help in what we're doing. What we certainly need is not to be hurt." So I'd like to propose that some thought be given to the kinds of hurt we do put on the private sector in research terms.

Also, I would like to express a personal prejudice here which is, I think that bibliometric procedures, to go back to some of OTA's comments, are really not very useful for your purposes, and I would like as a private sector participant here to discourage you from focusing too much on them. First of all, in certain disciplines of science—biotechnology, again, is a very good example—forcing people to measure their success by publishing actually cuts against private sector initiatives.

For example, one of my clients presently has a broad-scale anticancer compound in research. One of the research organizations working on it is M.D. Anderson, which is probably well known to all of you, and there is a very famous clinical investigator down there doing the work, and my client has asked him to refrain from publishing for a year until he can figure out if the patent on this compound is going to grant. Now how does this particular researcher feel if he happens to be in a year in which he's being appraised, and a number of his private sector clients whom he's aiding in research have prevented him from publishing? So I'd suggest to you that bibliometric research is something that I think the private sector would not react very well to as an overall measure.

Thank you, Mr. Chairman.

Are there any questions? I probably didn't provide you any more answers than any of your other witnesses, for which I apologize.

[The prepared statement of Ms. Tanner follows:]

Presentation to
The Committee on Science and Technology
U.S. House of Representatives

by

Mary C. Tanner
Managing Director
Shearson Lehman Brothers Inc.
"Federal Research Funding as an Investment"

April 29, 1986

April 29,1986

Good morning, Mr. Chairman, Gentlemen:

I would like to thank the members of the Committee for this opportunity to participate in discussion on a subject which is of immense interest to me and others in the private sector. I would also like to thank the Office of Technology Assessment for the quality and scope of briefing materials it has provided us. OTA's Technical Memorandum "Research Funding as an Investment: Can We Measure the Returns?" is in my view an excellent and very fair summary of a considerable amount of thinking on a complex and indeed confounding problem. As the memorandum properly reflects, private industry has not successfully answered the core question, any more than have government policy makers. There are considerable parallels within the private sector, as touched on in OTA's memorandum, to the study in which this Committee is engaged, and I think my best service to this Committee this morning will be to highlight that private sector perspective for you.

In taking this focus, I am not begging the fundamental study in which your Committee is engaged today: assessing overall return on federal research investment. In that regard, I would note that in order to define "returns" or "success" in any program in either the public or private sector, it is prima facie necessary to have a clearly defined goal. Given the size, scope and organizational structure of the Federal commitment to research, as well as the plurality of constituencies it serves, any singular or overriding goal cannot reasonably be set. Accordingly, no definitive methodology can be recommended for assessing return. Indeed, I would posit that this very plurality is one of the great strengths of our Federal research initiative compared to those in Europe or Japan, since it permits constituencies with different risk profiles, social perspectives (or lack thereof) and economic objectives to share research support. However, observers frequently define Federal research goals in three principal functional areas: aiding U.S. industry; substituting for U.S. private industry research and furthering broad or specific social goals.

My comments today will focus almost exclusively on methods of improving returns on Federal research where the goal is to aid or augment private sector research. Much of this type of research is in the "applied sciences": health care, electronics, etc. There are certain types of basic science (astronomy, particle physics) where Federal funding is the only realistic source of capital, given the enormous sums required for equipment and lack of any apparent practical economic or industrial application. These are not covered by my remarks.

Definition of research goals in the private sector is far easier than in the public sector. Generically described, the goal is production of commercially useable inventions, which can be successfully sold and produce a net increase in shareholder wealth, as measured in increasing company earnings, reflected in rising stock prices and dividends. The singularity of research goals in the private sector lends itself more easily to quantitative analysis than in the public sector. Nevertheless, even in the private sector, measuring returns on basic, versus applied research is difficult. The OTA memorandum properly notes that quantitative analysis of basic research in problematic, even for the private sector.

As a practical matter, in private industry, basic research is managed by a mixed system. An overall research budget is set in the general context of corporate financial performance. Either the Chief Executive Officer and/or the Director of Research divide the allocated research budget on a strict priority basis by reviewing projects nearest to commercialization. By definition, projects nearest to commercialization require the highest dollar amount of investment, and receive the most focus. Generally, top executives' focus on specific projects declines in accordance with dollar size of investment, except where research is expected to yield unusually large commercial benefits, regardless of time frame. Accordingly, basic research projects which individually require low investment are generally reviewed only by the director of research. Senior executive talent usually manages the research "mix" between "basic" and "applied" research on a rule of thumb basis peculiar to a company in the context of its industry. For example, a major pharmaceutical company would have 2% to 5% of its budget in true, early stage "basic research," 5% to 25% in "early products" and the remainder in products in various stages moving towards commercialization.

- The 2-5% segment would be managed at senior levels by general normative rules (in pharmaceuticals, how many active chemical compounds emerge as clinically interesting in a year, for example).

- The 5-25% segment would be managed by similar methods.

- It is only the 25-75% sector that is subjected to quantitative analysis.

The most generally accepted quantitative analysis method in the private sector is discounted cash flow analysis. Simply put, the cash expended in each period and cash received after commercialization is laid out on a time series of data points and "discounted" by an interest factor which represents the minimum required rate of return of the corporation on a particular category of investment. I will illustrate the use of this model later in my remarks.

The most important inputs to this model are fundamentally qualitative: assessment of the time and cost of research, costs of manufacturing capacity if any, investment in marketing, and, most importantly, sales and production costs estimates. When private sector commercialization of research "fails," it is generally because the all important steps between the research lab and commercial sales and time to accomplish these steps, has been inaccurately assessed.

The OTA memorandum provided to you highlights identical difficulties in using a similar type of quantitive analysis when assessing the overall impact of Federal research on the economy. I quote, from Chapter 2, p22-23 of the OTA study, regarding the utility of certain types of quantitative analysis for measuring R&D returns:

> "Another major stumbling block in econometric analyses is that they measure inputs (R&D investments) and outputs (productivity changes) while ignoring the process that goes on in between. That process is the critical stage of turning laboratory research into a tangible return -- innovation and commercialization. Research cannot result in product or process improvement unless each step in the move from idea to market is successful: advanced development, pilot studies, legal blessing of patents and licenses, production, and marketing. These intermediate commercialization steps are as important a factor in the move from R&D to productivity as is the R&D investment itself. However, the federal government has direct control only over its R&D investments, and very little influence over commercialization in the private sector." (emphasis added)

Indeed, the most powerful influences on commercialization of research in the private sector are those directly and indirectly exercised by the federal government through:

- fiscal and monetary policy and their impact on U.S. capital markets;
- trade policy;
- tax incentives;
- patent law; and
- regulation.

In terms of measurable influence on applied research productivity, these factors are at least as important, if not more important, than the direct Federal research dollar for most industries. These factors are largely beyond the control of private sector management, dramatically increase uncertainty and risk, and therefore lower return. I have listed these factors in the order of priority which my experience as an investment banker indicates is both the perception and reality in U.S. industry. Let me illustrate how powerful these can be in various industrial sectors by selected example.

Fiscal and Monetary Policy

o First, and most importantly, fiscal and monetary policy. In March of 1985, I spoke at an agricultural biotechnology conference at the University of California at Davis. At that time the public stock market prices of biotechnology companies were extremely low. My topic was to define the greatest challenges facing the biotechnology industry. I explained to my audience that the greatest challenge, and the solution to it, were completely beyond the capacity of the biotechnology to confront or control. That challenge was a strict, financial return on investment criterion, caused by the level of interest rates on Federal borrowings. At that time, the rates of interest on 10 year and 30 year Treasury bonds were approximately 11.80%.

These rates are generally used by investors in the stock market as the so-called "risk free rate." An investor, facing a choice of investment alternatives, will compare a "risk free" investment like U.S. government bonds to his alternatives, in this case, biotechnology stocks. The rational investor will demand a higher return than that he receives in Treasury bonds on other, riskier, investments. Biotechnology, at its then stage of commercial development, was among the "riskiest" of investments one could make. My estimate in March of 1985 was that investors were demanding a three-fold premium to U.S. Treasury bonds in order to invest in biotechnology, or about 35%. Accordingly, stock prices for these companies were very low, since despite their dramatic expected commercial progress over the next 5 to 10 years, the required rate of return on investment by public and private investors reduced the present investment value to very little.

The situation is very different today. As of Friday, the 10 and 30 year Treasury bonds carried interest rates of roughly 7.60%. Our public stock markets are at new highs, reflecting an expectation of continued low inflation and relatively stable interest rates. Reflecting the substantial drop in the "risk free" rate on government securities, the required rate of return on "risky" investments like biotechnology stocks has fallen to about 18 to 20% -- still about a three-fold premium to U.S. Treasury bonds, but dramatically lower than the 35% appropriate in 1985.

I would submit Tables 1 and 2 to members of the Committee as simple illustrations in numbers of the point. Table 1 takes the perspective of an investor in a biotechnology stock in the spring of 1985 versus 1986. Table 2 takes the perspective of an executive at a biotechnology company evaluating the return to his stockholders on investment in two new pharmaceutical compounds during those same periods.

Table 1 illustrates the effect on stock prices of the very different capital market situations in early 1985 and 1986, caused in large part by differences in U.S. fiscal and monetary policy. The practical implications of this table to a biotechnology company, or this Committee, are relatively simple. In early 1985, to raise $50 million, it would have taken 3.3 million shares of this stock, and could perhaps not have been done, even if management and shareholders had been willing to give away that much of their company. Today, to raise the same amount of money would have required 2.3 million shares. The lesson for this Committee is several fold. I will touch on only two points:

> First: One of our greatest national assets is the incredibly deep, broad and flexible U.S. capital market, which offers particular advantages for companies raising equity or funds for long-term, permanent investment compared to those in Europe or elsewhere. Substantial sums are available in this market to finance even risky, early stage research-oriented enterprises, such as biotechnology. However, when a high degree of uncertainty regarding fundamental fiscal and monetary policy, and particularly inflation, exists, higher risk investments are "crowded out" or "priced down" in the market. This has direct and negative bearing on capital formation for innovation-intensive industries or industry sectors.
>
> Second: Access to reasonably and consistently priced capital is a critical input to successful research as well as transition from research to commercialization. One of the important comparative advantages of Japanese companies is their favorable access to large domestic pools of savings at very cheap and historically stable rates (about 3.5% today) to finance industry.

Table 2 illustrates this point. This table shows a series of expenditures and eventual profits from two potential new biotechnology compounds for the pharmaceutical industry, both potential anticancer compounds under development at a number of companies. You can see that an investment of $77.0 million is required before commercial introduction. You can also see the different net present value, or value in 1986 dollars, from the profits shown in years 1991 through 1995, at the different 1985 and 1986 rates of return, 35% and 25%, respectively, I have discussed earlier. In contrast, a Japanese company would have been evaluating this investment at the same rates of return in both years because of the stability of their financial markets in recent years compared to ours. Accordingly, the Japanese would have made the typical "more rational, long-term" investment choice for which they are so widely praised. I would posit that, from the luxury of that Japanese manager's position, so would an American management.

In conclusion, the most important impact the Federal Government has on the vigor and success of applied research in U.S. industry and the economy as a whole is through the mechanism of fiscal and monetary policy. While this issue affects all of U.S. industry, nowhere is its impact more obvious than in high technology and emerging industries, which are "risky" investments, and where dramatically increasing the required rates of return, as a function of the rate at which the U.S. Treasury borrows, has the greater proportional impact compared with other industrial types of investment. In 1984 and through June of 1985, the U.S. biotechnology industry raised less than $75 million publicly in U.S. equity markets. Since September of 1985, through today, which corresponds with dramatic declines in interest rates, U.S. and foreign biotechnology firms have raised $300 million. The OTA memorandum provided to you notes, with reference to a number of studies, that while the decades of the 1950's and 1960's produced consistently high levels of investment by U.S. industry in both basic and applied research, the 1970's showed a significant slowing of this investment flow because industry became "skeptical of a lack of demonstrable return." Needless to say the 1970's and 1980's were marked by profound economic and capital market disruptions and persistently high interest rates and inflation. For this environment only short-term investments "pay off" in industry -- which automatically mediates against stable, long-term investment in research. I would like to ask members of the Committee to retain Table 2 at hand, since it will be important to one of my later comments.

Trade Policy

The second major indirect, or direct, impact the Federal government has on applied research productivity is in trade policy.

I should make plain at the outset that I do not advocate erecting major new protectionist barriers, since the illustration of the highly structured and fundamentally weak economies of Germany, Sweden, France and England provide adequate illustration of the long-term detractions of this strategy. I do, however, applaud the ruthless and energetic prosecution of our rights under established trade treaties. This is, of course, a highly complex and controversial subject. I will choose only two examples. An easy example, indeed, is the present Administration's recent persistent prosecution of antidumping procedures against Japanese semiconductor manufacturers. This procedure is an ideal mechanism for enforcing the stability and integrity of our domestic markets, and one which I applaud vociferously. I also regard the semiconductor industry as a strategic, and indeed military, resource of this country. The semiconductor industry also feeds one of our largest and historically most successful industries, the computer industry. You will find opinion on both "the problem of" the semiconductor industry and "the solution" thereto divided, both in the industry and among its customers. Since semiconductors are highly capital intensive, the industry suffers from the same return on investment and capital market issues I described above in the context of the biotechnology industry. Added to this is the unfairness of competing with generous depreciation and tax advantages of which Japanese manufacturers in this industry can avail themselves. Finally, there is the problem that the Administration's prosecution initiative comes almost too late to save large parts of the industry from what allegedly has been unfair foreign trade practices. This is, of course, the view of the semiconductor industry. The view of many consumers of semiconductor devices differs. Their call is that, since these devices form such an important part of cost in computers, the best long-term interest of U.S. computer companies and U.S. consumers is served by the cheapest source of devices, whether Japanese, U.S. or Taiwan based. Maybe so, but I would note that a substantial body of counterweight opinion exists, too complex to present today.It is my view that long-term government based initiatives such as certain of those undertaken by DARPA (Defense Advanced Research Projects Agency) are essential to supplementing and encouraging applied research productivity in this important industrial sector. However, in the short-term, vigorous prosecution of legitimate antidumping cases, or other initiatives to enforce a level of reality in reciprocal trade terms, is essential in many high technology industry sectors.

A second example, more briefly stated, is in the pharmaceutical area. The amount of time required by the Japanese patent office to approve a local patent on a new foreign pharmaceutical compound is dramatically longer than the U.S. Patent Office's reciprocal treatment. Complaints on this subject are widespread and of long standing among U.S. and foreign pharmaceutical manufacturers. There are numerous industry hearsay stories about local Japanese patents applied for, subsequent to a U.S. company's submission, being approved earlier by Japanese patent authorities. Since the proprietary protection provided by patents is essential to recovery of investment in research and clinical testing, and because Japan is the second largest pharmaceutical market in the world, this problem directly affects returns on research by U.S. pharmaceutical companies. U.S. government trade initiatives in this area could produce direct industrial benefits. It could also improve, indirectly, return on U.S. taxpayers' research dollar, since many fruitful pharmaceuticals result directly or indirectly from basic and applied research funding by the National Institutes of Health, NCI, etc.

Tax Policy

The third major indirect, or direct impact the Federal government has on applied research productivity is through tax policy. This is among the most complex and controversial of subjects, particularly in this legislative year. However, it is clear that philosophically, socially, and as a policy matter we do actively incentivize various industrial sectors through tax policy. It is curious to me that, given the scope of the Federal budget devoted to research, and the relative efficiency demonstrated over the last 30 or 40 years of tax incentives in directing investment flows in the U.S. economy, little focus has been given to the potential benefits of increasing incentive for private sector research rather than capital investment, through tax policy.

Enhancement, or at least preservation of the research and development tax credit, and reorientation of its structure, could be an important and extremely useful legislative initiative of this Committee. In its present form, the credit sharply favors larger, rather than smaller industrial enterprises, where, in fact, a substantial amount of the most innovative applied research has been accomplished in recent years. Internal Revenue Service treatment of research and development limited partnerships, and the possibility of passing on excess corporate R&D credits to individuals in these financing structures also merits careful study. My Firm has undertaken considerable study of this issue, which I would be pleased to provide to Committee members at a later time, should this be of interest.

Patent Law

A fourth major area of direct federal influence on the success of applied research is patent law. U.S. industry generally, by my somewhat unscientific observations over 10 years in finance, gives the U.S. Patent Office high marks for their thoroughness, and more particularly, their willingness to rise to the challenge of radically new technologies. The extraordinary focus and continual increase in sophistication exhibited by the Patent Office over the last 10 years regarding patents in the biotechnology industry is an excellent example of superior performance. However, in certain areas such as semiconductors, and software, to pick only two, more focus is required. Protection of intellectual property rights for some period of time sufficient for industry to recover its investment in research is essential to fostering vigorous applied research. Finally, clear, incontrovertible assignments of patents received under Federally funded programs to industrial enterprises for commercialization is a keystone to fostering useful application of these inventions. Any weakening of the current procedures, to require broad based compulsory licensing under the aegis of "competition" will generally only decrease return on the U.S. taxpayer's research dollar by providing lower incentives for companies to commercialize these inventions.

Regulation

This is an immensely complex and varied topic of discussion, even when focused exclusively on the impact of R&D on the success or failure of applied research by U.S. industry. I would therefore like to focus only on two vignettes as illustrative of the enormous potential impact of regulation.

First, if members of the Committee will retrieve Table 2 and add next to it Table 3, you will see a vivid illustration of the financial impact of two years delay in approval by the Food and Drug Administration of the biotechnology pharmaceutical anticancer compounds I discussed earlier. Table 2 illustrates the return on the project or net present value, that is, value in 1986 dollars of approval in 1991 ($150-500 million). Table 3 shows the value of the project if approval is achieved two years later ($19-103 million). You can see that the value is materially less with later approval.

Similar data on agricultural biotechnology projects would yield similar adverse comparisons from delayed approval by the U.S.D.A.

While legislative initiatives in this area by this Committee may perforce be limited by the Agency structure of the U.S. government, in your assessment of technology you should be aware that project investment choice in private industry is frequently heavily dictated by the perceived uncertainty of regulatory approval processes particularly in human and animal health care and agricultural research.

Thus in summary, it is my view that selected legislative initiatives by this Committee in the direct and indirect areas of the Federal government's influence on the success of research in the private sector, could produce measurable results by stimulating private sector investment in research, and increasing the rate of commercialization of Federally funded research. Among the several initiatives I noted, I would particularly emphasize fiscal, tax and trade policy, followed by patent legislation.

Further, a useful initiative, perhaps using the resources of this Committee, into strategic areas of industrial research investment, could provide a platform to channel some of the existing efforts of many diverse Federal research funding sources. Widespread commentary in U.S. industry on the need for focused goals and directed funding in so-called "strategic" research areas has resulted from the highly publicized efforts of the Japanese Ministry of Trade and Industry and other market related forces. While many of these comments are in fact thinly-masked cries for protectionism, it is my view that concerted study of the subject would produce useful information, including the fact that U.S. industry is underinvesting in certain long-term applied research technologies, where immediate "payoff" (i.e., 5 to 10 years) is not evident, or where the costs of development are prohibitively large, even for our most substantial Fortune 100 companies. A few areas that appear to fit this description are:

- materials science, particularly composites and ceramics to be used in substitution for metals in industry;

- advanced device physics and material science for the semiconductor industry;

- artificial intelligence and parallel data processing technologies, which many expect will revolutionize the computer industry, and which are the subject of a widely publicized initiative by the Japanese.

- gerontology, with particular emphasis on medicine and health care.

- agricultural and industrial (chemical, paper and process industry, waste management) applications of biotechnology. While NIH, NCI and other funding sources for human and animal health uses of biotechnology are presently adequate, industrial applications are underfunded.

The method of identifying these "under invested" technologies should be the "bottoms up" approach attributed to Japan's Ministry of International Trade and Industry, described by OTA in its memorandum. Consultation by this Committee with U.S. industry, identifiable research leaders in particular technologies, Noble laureates and members of the National Academy of Sciences should be broad based, and synthesized into a mosaic of areas where, despite our substantial Federal commitment to research, we may be "missing" important opportunities. The reasons, if identifiable, that these areas are being missed should be particularly closely examined. Is it an overlapping U.S. agency problem, a lack of sponsorship in the logical funding area, etc.?

Direct initiatives in this area may be difficult to implement depending on the results of the analysis. Nevertheless, the most practical result may be a framing of the problem for Congress and U.S. industry, and an opening of the debate as to whether such funding is required or even desirable.

I would like to thank the members of this Committee for their time and courtesy, and take any questions you may have.

TABLE 1

ANALYSIS OF STOCKHOLDER'S RETURN ON INVESTMENT IN COMPANY A BIOTECHNOLOGY STOCK

REQUIRED RATE OF RETURN	35%	18-20%
STOCK PRICE IN 3 YEARS (TO CREATE REQUIRED RATE OF RETURN)	$37	$37
RESULTING STOCK PRICE TODAY	$15	$21-22
% DIFFERENCE		32%

TABLE 2

THERAPEUTICS VALUATION

PROJECTIONS

($ MILLIONS)

		COMPANY	
	REVENUES	PRE-TAX	CASH FLOW
1986	--	$ (2.9)	$ (2.9)
1987	--	(6.1)	(6.1)
1988	--	(15.2)	(15.2)
1989	--	(20.5)	(20.5)
1990	--	(13.8)	(14.1)
1991	$ 6.1	(12.0)	(18.2)
1992	74.6	31.7	3.2
1993	207.5	126.4	28.7
1994	394.1	254.9	50.8
1995	589.1	376.2	109.1

RETURN ON INVESTMENT AT RATES OF: (A)

20%	$496.9 MILLION
35%	$148.9 MILLION

CASH INVESTED $77.0 MILLION

(A) INCLUDES RESIDUAL VALUE

TABLE 3

THERAPEUTICS VALUATION

PROJECTIONS

($ MILLIONS)

	REVENUES	PRE-TAX	CASH FLOW
1986	--	$ (2.9)	$ (2.9)
1987	--	(6.1)	(6.1)
1988	--	(15.2)	(15.2)
1989	--	(20.5)	(20.5)
1990	--	(13.8)	(14.1)
1991	$ 6.1	(12.7)	(18.9)
1992	19.5	(10.2)	(15.2)
1993	35.5	(13.4)	(22.1)
1994	94.8	11.9	(14.1)
1995	217.8	61.1	(0.4)

RETURN ON INVESTMENT AT RATES OF: (A)

20%	$103.4 MILLION
35%	19.3 MILLION

CASH INVESTED $129.6 MILLION

(A) INCLUDES RESIDUAL VALUE

DISCUSSION

Mr. **Fuqua**. Thank you very much, and from a very interesting perspective. Of course much of industry is involved in applied research. Pharmaceutical companies are trying to develop new drugs.

Ms. **Tanner**. Yes.

Mr. **Fuqua**. You mentioned some of the materials science advanced device physics for the semiconductor industry, artificial intelligence, gerontology, agriculture, industrial chemical, and so forth, and I agree that probably bibliometrics have some problems, particularly if you're talking about in engineering and applied research, because a lot of that doesn't get into literature.

Ms. **Tanner**. Correct. That's a very well taken point.

Mr. **Fuqua**. So then you get into kind of a numbers game of how do you distribute the available funds. Do you have an alternative to that?

Ms. **Tanner**. I think that if we are——

Mr. **Fuqua**. Because we are trying to be helpful.

Ms. **Tanner**. I understand.

I think if one could posit that we were entering a period of stable interest rates and a sound, if you will, and relatively thriving economic profile—such as our capital markets right today, I think we have—you could posit that very substantial sums of private capital would be available to those late applied research stages, and you could also posit that you could therefore go back into various agency budgets and focus quite strictly on what their financing that is really applied versus what is really basic.

NIH, for example, I don't think I would qualify all of that budget as basic research; in fact, far from it. Now NIH funding, quite frankly, of clinical trials for biotechnology pharmaceuticals over the last 2 years, when there has been absolutely no public equity market financing available to these companies, has meant the difference between life and death of about 25 different commercial enterprises that will, in another 2 or 3 years, be very sizable pharmaceutical companies.

So tell me what the economy looks like, and one can make a series of measures—series of recommendations.

I do think that the most critical need industry has is for basic research, but basic research is a very tough word to define, and companies carry on more of it than you probably think they do.

Let me give you an example of a sort of basic research problem. Artificial intelligence, which is in a number of the industries in which I have specialized over the years, was actually and has been available in the literature for almost 30 years now. It hasn't been usable by anybody, even the sort of enormous commercial computer enterprises that you're so familiar with, because the barrier to commercialization was that we didn't have cheap enough hardware; it wouldn't run fast enough to really be useful, if you will, as a replacement for certain kinds of human activity. Well, 5 years ago, computers got fast enough and cheap enough so that now artificial intelligence can be adopted, and in fact that is the kind of thing that the "Traces" study has looked at over the years.

If you look at artificial intelligence today, between DARPA—particularly DARPA—and a number of other sort of pockets of Federal money, there is both basic and applied research funding going on. Which is more important to my industrial clients? Probably DARPA, I think, has been the most creative agency I've seen, other than NIH, over the years, in identifying basic research projects that can be funded directly into industry and then allow them to take the next step on from it. But DARPA is relatively new, and I don't think its examples are well applied because, again, it proceeds from the view of national defense objectives.

Mr. **FUQUA**. We had—the committee participated in a symposium, I guess a couple of years ago, in trying to encourage the commercialization of Federal military research that doesn't violate national security interests.

Ms. **TANNER**. Sure.

Mr. **FUQUA**. And, of course, DARPA would figure in that among several of the other agencies that were in the Department of Defense. So that's something we've tried to encourage in making the information available.

Ms. **TANNER**. I think, by the way, in certain areas like materials science, some effort to get great diffusion of some of the products of defense-based research would be meritorious, but it depends on the structure of the industry. I think OTA was very wise in pointing out, for example, why agricultural federally funded research has worked so well—the structure of the industry.

The problem in the pharmaceutical——

Mr. **FUQUA**. And it's probably been the oldest.

Ms. **TANNER**. Yes.

Mr. **FUQUA**. As far as funding the longest period of time.

Ms. **TANNER**. That's probably right.

Although in talking about Federal agricultural research, you also have to talk a little bit about subsidy programs as well. It's a more complicated subject, I think, than sometimes it appears.

But in the pharmaceutical industry, you could not have the same kind of research initiatives because the costs of the type of research that has to be done by private industry are different. The cost of a new drug today is somewhere between $25 and $50 million, and it takes a random amount of time between 5 and 12 years—not very helpful if you're a private executive. Some of that time, I might add, comes from hopeless overburdening at the FDA where applications will sit on people's desks for 2 years because they just can't get to them, or because as a policy matter the FDA has decided that other things have higher priority.

When you're investing $25 and $50 million, you've got to have patent protection. Therefore, certain of the diffusion policies that NIH has practiced, or proposed to pursue, over the years are counterproductive to commercialization of those research findings, because nobody can get a proprietary position and therefore assure themselves of a return that makes it worthwhile to risk $50 million in clinical research on 2,000 patients.

So what would work perhaps in materials science won't work in pharmaceuticals. It would work probably in the computer industry, where the timeframe between basic research and commercialization is so short and the amounts are not that large so that you could have broad diffusion of Federal research without risk that it would not be commercialized because there's no proprietary position.

Now I'd also suggest to you, by the way, that my argument on the computer industry that I've just made is somewhat circular. It's somewhat circular because there never has been proprietary protection for anything. So it's fairly hard to tell what would have happened had there been, and Japan, for example, provides its various computer companies with a sort of de facto kind of proprietary protection, although it doesn't have distinctive patents by the rigorous enforcement of certain kinds of policies by MITI and other government agencies which are not possible in this country.

So there aren't a lot of easy answers to your questions. It's a wonderful goal to try to diffuse Federal research. In certain industries would be counterproductive; in other industries, it would be extremely productive. I think a study group on the structure of industries and direct contact with U.S. industrial companies of some technology prominence, who presumably have as great a stake in this, if not a greater stake in some ways, than taxpayers, at least directly, might be helpful to you—understanding different industry sectors, what's important to them in terms of being able to take Federal research, than stepping back and looking at where the broadest base of funding for those various technologies and industries are. You might be able to define some objective recommendations for improving commercial use of Federal research. But it has to be done on an industry-by-industry basis; otherwise it's useless.

That is, in fact, one of the problems that I was trying to identify for you in talking about tax policy or patent law. Broad initiatives of that sort can frequently cut very deeply across certain industries even if they appear to be relatively beneficial overall.

Mr. FUQUA. You indicated on the bottom of page 11 about some study that you firm had done on R&D tax credits.

Ms. TANNER. Yes.

Mr. FUQUA. Could you provide that to the committee?

Ms. TANNER. I'd be delighted to. Unfortunately, I did not bring it with me today, but I will send it to you.

Mr. FUQUA. That's fine.

Mr. Slaughter.

Mr. SLAUGHTER. No questions, Mr. Chairman.

Mr. FUQUA. Thank you.

Ms. TANNER. Thank you.

Mr. FUQUA. Thank you very much, Ms. Tanner.

I'm going to have to excuse myself and will turn the meeting over to Mr. Slaughter.

Mr. SLAUGHTER [acting chairman]. Dr. Olesen.

Dr. Bagley, we'd be glad to have you as a substitute for Dr. Olesen.

Dr. BAGLEY. All right.

STATEMENT OF DR. JOHN F. BAGLEY, MANAGER, INSTITUTIONAL RELATIONS, BATTELLE PACIFIC NORTHEAST LABORATORIES, WASHINGTON, DC; FOR DR. DOUGLAS E. OLESON, EXECUTIVE VICE PRESIDENT, BATTELLE MEMORIAL INSTITUTE, COLUMBUS, OH

Dr. BAGLEY. Let me first say I apologize on behalf of Dr. Olesen for his not being here today. He had a personal situation which required that he not be here this morning. So where the word "I" appears in here, please understand that it's "we."

Mr. Chairman and members of the task force, on behalf of Battelle Memorial Institute, I appreciate the opportunity to appear before the task force. As you may know, the principal component of Battelle's business is research and development, and we have been following with particular interest the Science and Technology Committee's efforts at evaluating the science policies of the United States. I applaud your efforts and hope the views I will be sharing with you today will in some way contribute to the task force's goals. I should add that my comments are reflective of an organization that was created through the vision of an Ohio industrialist, an entrepreneur, who in 1923 directed that the bulk of his estate be used for the encouragement of creative research and the making of discoveries and inventions.

I'll go to the middle of page 2.

The question is "Have there been sizable payoffs from the Federal investment in R&D?" I think all would agree that there have been significant advancements in science and in our standard of living from research sponsored by the Federal Government. The current understanding of elementary forms of matter and the universe as a whole; the very existence of the aerospace, computer, microelectronics, and information processing industries; and the continually increasing life expectancy of the American people are only a few examples where Federal research expenditures have benefited the United States and its economic and societal development. The real question is, "Are we getting as much of a payoff as we should commensurate with the level of expenditures?" As you have heard from other witnesses, there is no magic answer to that question, I don't think. My personal, intuitive feeling is that the Nation has done rather well on its investment in research. It has been my experience that most scientists and engineers engaged in research have been both very conscientious and productive in their work. If we, as a Nation, have gotten less than we should for our research expenditures, the problem, in most cases, is that we did not clearly identify what we expected and wanted for our R&D dollars; or, we, as a Nation, did not provide the direction and continuity of support needed to bring a research project or program to a successful conclusion. The real key to maximizing the benefits of Federal research expenditures is overall policy and funding stability. Sizable swings in the level of Federal expenditures, extended policy debates, or radical policy changes are not conducive to effective research results. In fact, it's probably unrealistic to expect significant results from research projects that are stretched out or terminated early for nonscientific reasons, or from research programs that are the subject of continuing policy debates. Let me give you some examples.

Contrast the debate within the Government on the nuclear breeder reactor or the SST with the consensus agreement among the executive and legislative branches on DOE's high energy physics program, NASA's space exploration program, or NIH's health research programs. On the one hand, the Government ultimately received a limited return from hundreds of millions of dollars invested because of an extended debate over the purpose and merit of the program while, on the other hand, there have been significant benefits from those programs not affected by these sharp disagreements and are characterized by support from all branches of Government. Moreover, there will continue to be benefits from these established, well-supported programs. We just don't see them yet. I suggest that we keep these contrasts in mind as the Federal Government proceeds with the SDI research program, with the so-called Orient Express program recently announced by the President and supported by the Congress, and with the Superconducting Super Collider, which is still in the debate stages, especially in today's constrained budget situation.

How do we measure the impact of Federal R&D? As the task force has already heard from other witnesses—and I think on behalf of Battelle we agree—there have been various criteria used over the past years to attempt to measure the success or progress of research supported by the Federal Government. These criteria include the number of patent applications and patents awarded; the number of scientific articles and citations; the number of students enrolled or the number of advanced degrees awarded in science and engineering curricula; the number of scientists and engineers engaged in R&D as a percent of the labor force; the level of high-tech exports; or even the number of high-tech stock offerings.

It is easy to point to the fact that supercomputers can process bits of information in nanoseconds; that machines can be designed and manufactured to perform routine work once only performed by humans; that man can fly to the Moon and safely return to Earth; that most communicable diseases have been eradicated from all our advanced countries, et cetera. But, there is no plausible way that we can show with statistical data that the benefits from each of these scientific advancements have been attained at less cost or could have been attained at less cost.

Since Battelle performs research for both Government and industrial sponsors, it may be useful for some analogies to be made. In both Government and industry, an element of an organization identifies a need for additional research. This data, if obtained from this research, may be useful in improving output, improving a product, making a new product, counteracting an existing impediment to performance, et cetera? Eventually, a request for funds to perform research for a variety of identified needs gets to a decisionmaking body. The decisionmaker is ultimately the board of directors in industry or the Congress. After some deliberation, a level of funding is deemed appropriate, but no one really knows whether the research will yield the result one is hoping for. There is a belief that the expenditure will somehow or other be worth the investment. A board of directors or Congress looks to the various other parties in that organization to help ensure that progress is being made. Battelle participates in program reviews, financial reviews, investment strategy reviews, management reviews, involving both Government and industrial research programs. Through these reviews people have to make judgments as to whether the previous decisions are still valid, whether some changes are necessary, or whether the research program seems to be yielding any significant results. These judgments and decisions are many times aided through a peer review process.

Investment in research, whether by Government or industry, must be, I think everyone agrees, accepted as a risk. There have been enough new ideas, new inventions, new processes developed through research programs that I believe most managers and business executives are convinced that the investment is not only generally worth the risk but mandatory to remain competitive. The only question is, what is the appropriate level of investment, taking into account a range of other factors such as profitability and competition; and I think Ms. Tanner alluded to those in much more detail.

So it should be with Congress, too. By the time the research budget requests of the various agencies get to you, a series of management review processes have already occurred and will continue on a routine basis. I would suggest that Congress must make the same value judgments as a board of directors in the private sector, taking into account the total budget of the Federal Government. What is the level of research that the Federal Government can afford within a larger realm of revenues, expenditures, GNP, et cetera? Equally important, and accepting the fact that research has proven to be a good investment, what does a reduced level of research mean to the competitiveness of American industry or the well-being of our citizens? Congress ultimately must make a value judgment on the level of funding as a whole and, through various subcommittees, a series of decisions on funding levels for various research programs. And, you have to make these decisions with little quantitative data. There is frustration, but in my view there is little hope of ever getting the kinds of quantitative or statistical information to justify one way or another your decisions.

Evaluation of the benefits of science is really an "unscientific" field. Despite the fact that a number of highly respected economists, some of whom you will hear from later, some of whom you have already heard from, have devoted a great deal of study to this issue, I believe this is an area where we just have to recognize that it may not be possible to measure with any degree of precision what the specific benefits of a research program are.

Having said this, I think one should recognize that while we may not be able to quantitatively measure the results of Federal research efforts to our satisfaction, it is still useful for us to continue to consider such larger indexes as economic growth, international competitiveness, balance of trade, new businesses, and the number of scientists and engineers in the work force as barometers of our Government's successful or failed R&D programs.

There is another element to this discussion that we believe also needs to be noted. Over the past 4 years, largely at the Federal Government's initiative, there has been greater cooperation between Government and industry in working together toward developing programs aimed at enhancing U.S. competitiveness in the world global economy. The Government is seeking the input of industry in developing research programs to address specific needs such as the recent initiative to improve the competitiveness of the American steel industry. In addition, a study by the White House Science Council, properly referred to as the Packard Committee Report, has charged the Government with aggressively transferring the results of the Federal research conducted in its laboratories to the private sector and encouraging the use of sophisticated equipment in the laboratories by industry. I believe positive results in this new cooperation are already being seen in a variety of fields.

In conclusion, despite the difficulty of measuring the benefits of research, we believe that our country's investment in research has paid off handsomely. More precise means for measuring this payoff are certainly a desirable goal, but we do not believe we can afford as a Nation to slow down the pace of research as we search for some sophisticated measurement techniques to measure the benefits of this research.

On behalf of Battelle, thank you, Mr. Chairman.

[The prepared statement of Dr. Olesen follows:]

STATEMENT OF

Dr. Douglas E. Olesen
Executive Vice President
Battelle Memorial Institute

before the

Science Policy Task Force
Science and Technology Committee
U.S. House of Representatives
Washington, D.C.

April 29, 1986

Introduction

Mr. Chairman and Members of the Task Force. On behalf of Battelle Memorial Institute, I appreciate the opportunity to appear before the Science Policy Task Force today. Since the principal component of Battelle's business is research and development, we have been following with particular interest the Science and Technology Committee's efforts at evaluating the science policies of the United States. I applaud your efforts and hope the views I will be sharing with you today will in some way contribute to the Task Force's goals. I should add that my comments are reflective of an organization that was created through the vision of an Ohio industrialist and entrepreneur who in 1923 directed that the bulk of his estate be used "for the encouragement of creative research and the making of discoveries and inventions."

Background

Battelle is the world's largest independent contract research and development organization. In any given year, our laboratories in this country and Europe have underway several thousand research tasks involving a broad cross-section of scientific disciplines. The principal sponsor of this research in the U.S. Government (DOE, DOD, EPA, NASA, NIH, DOT) -- $390 million in 1985. We also perform extensive research for many U.S. industrial firms (iron and steel, automobiles, chemicals, utilities, information processing, etc.). In addition, Battelle's two laboratories in Europe perform research for several foreign governments and many industries.

Your letter of invitation requested my views on the payoff from scientific research sponsored by the federal government, and measures that are available to determine whether federal expenditures on research are producing the desired results.

The focus of my remarks today will be on the total resources being expended for research and development without regard to the various phases of R&D or the split between defense and non-defense sponsorship of these expenditures.

Payoffs from Federal Research

Have there been sizeable payoffs from the federal investment in R&D? I think all would agree that there have been significant advancements in science and in our standard of living from research sponsored by the federal government. The current understanding of elementary forms of matter and the universe as a whole; the very existence of the aerospace, computer, micro-electronics, and the information processing industries; and the continually increasing life expectancy of the American people are only a few examples where federal research expenditures have benefited the United States and its economic and societal development. The real question is: are we getting as much of a payoff as we should commensurate with the level of expenditures?

I do not believe there is any way to answer that question with certainty. My personal, intuitive feeling is that the nation has done rather well on its investment in research. It has been my experience that most scientists and engineers engaged in research have been both very conscientious and productive in their work. If we, as a nation, have gotten less than we should for our research expenditures, the problem, in most cases, is that we did not clearly identify what we expected and wanted for our R&D dollars. Or, we, the nation, did not provide the direction and continuity of support needed to bring a research project or program to a successful conclusion. In my view, the real key to maximizing the benefits of federal research expenditures is overall policy and funding stability by the government. Sizeable swings in the level of federal expenditures, extended policy debates or radical policy changes are not conducive to effective research results. In fact, it is probably unrealistic to expect significant results from research projects that are stretched out or terminated early for non-scientific reasons, or from research programs that are the subject of continuing policy debates. Let me give you some examples.

Contrast the debate within the government on the nuclear breeder reactor or the SST with the consensus agreement among the executive and legislative branches on DOE's high energy physics programs, NASA's space exploration program, or NIH's health research programs. On the one hand, the government ultimately received a limited return from hundreds of millions of dollars because of extended debate over the purpose and merit of the program, while on the other hand there have been significant benefits from those programs not affected by sharp disagreements and characterized by support from all branches of government. Moreover, there will continue to be benefits from those established well-supported programs. I suggest that we keep these contrasts in mind as the federal government proceeds with the SDI research program, with the so-called "Orient express" program, and with the Superconducting Super Collider (SSC)--especially in view of today's constrained budget situation.

How to Measure the Benefits from Federal Research

How do we measure the impact of federal R&D? As the Task Force has already heard from other witnesses, there have been various criteria used over the past several years to attempt to measure the "success" or "progress" of research supported by the federal government. These criteria include the number of patent applications and patents awarded; the number of scientific articles and citations; the number of students enrolled or the number of advanced degrees awarded in science and engineeering curricula; the number of scientists and engineers engaged in R&D as a percent of the labor force; the level of high tech exports; the number of high tech stock offerings, etc.

It is easy to point to the fact that supercomputers can process bits of information in nanoseconds; that machines can be designed and manufactured to perform routine work once only performed by humans; that man can fly to the moon and return safely to earth; that most communicable diseases have been eradicated from all advanced countries, etc. But, there is no plausible way that we can show with statistical data that the benefits from each of these scientific advancements could have been obtained at less cost.

Since Battelle performs research for both government and industrial sponsors, it may be useful for me to make some analogies that we see. In both government and industry, an element of the organization identifies a need for additional research. This data, if obtained, may be useful in improving output, improving a product, making a new product, counteracting an existing impediment to performance, etc. Eventually, a request for funds to perform research for a variety of identified needs gets to a decisionmaking body. This decisionmaker is ultimately the Board of Directors in industry, or the Congress. After some deliberation, a level of funding is deemed appropriate, but no one really knows whether the research will yield the results one is hoping for. However, there is a belief that the expenditures will be worth the investment. A Board of Directors or Congress looks to various other parties in the organization to help ensure that "progress" is being made. We at Battelle participate in program reviews, financial reviews, management reviews, etc., for both government and industrial research programs. Through these reviews people have to make judgments as to whether the previous decisions are still valid, whether some changes are necessary, or whether the research program seem to be yielding any significant results. These judgments and decisions are many times aided through a peer review process.

Investment in research, whether by government or industry, must be accepted as a risk. There have been enough new ideas, new inventions, new processes developed through research programs that I believe most managers and business executives are convinced that the investment is not only generally worth the risk but mandatory to remain competitive. The only question is--what is the appropriate level of investment taking into account a range of other factors such as profitability and competition.

So it should be with Congress too. By the time the research budget requests of the various federal agencies get to you, a series of management review processes have already occurred, and will continue on a routine basis. I would suggest that you have to make the same value judgments as a Board of Directors in the private sector, taking into account the total budget of the federal government. What is the level of research that the federal government can afford within the larger realm of revenues, expenditures, GNP, etc.? Equally important, and accepting the fact that research has proven to be a good investment, what does a reduced level of research mean to the competitiveness of American industry or the well-being of our citizens. Congress ultimately must make a value judgment on the level of funding as a whole, and through various subcommittees, a series of decisions on funding levels for various research programs. And, you have to make these decisions with little quantitative data. I understand your frustration, but in my view there is little hope of ever getting the kinds of quantitative or statistical information to justify one way or another your decisions.

Evaluation of the benefits of science is an "unscientific" field. Despite the fact that a number of highly respected economists, some of whom you will hear from later, have devoted a great deal of study to this issue, I believe this is an area where we just have to recognize that it may not be possible to measure with any degree of precision what the specific benefits of a research program are.

Having said this, I should point out that while we may not be able to quantitatively measure the results of federal research efforts to our satisfaction, I believe it is still useful for us to continue to consider such larger indexes as economic growth, international competitiveness, balance of trade, new businesses, and the number of scientists and engineers in the work force as barometers of our government's successful or failed research and development programs.

There is another element to this discussion that also needs to be noted. Over the past four years, largely at the federal government's initiative, there has been greater cooperation between government and industry in working together toward developing programs aimed at enhancing the U.S. competitive position in the global economy. The government is seeking the input of industry in developing research programs to address specific needs, such as the recent initiative to improve the competitiveness of the American steel industry. In addition, a study by the White House Science Council (The Packard Committee Report on the Federal Laboratories) has charged the government with aggressively transferring the results of federal research conducted in its laboratories to the private sector and encouraging the use of the sophisticated equipment in these laboratories by industry. I believe positive results of this new cooperation are already being seen.

Summary

In conclusion, despite the difficulty of measuring the benefits of research, I certainly believe that our country's investment in research has paid off handsomely. More precise means for measuring this payoff are certainly a desirable long-term goal, but I do not believe we can afford, as a nation, to slow down the pace of research as we search for sophisticated measurement techniques.

Again, thank you for this opportunity to present these views.

Dr. Douglas E. Olesen
Executive Vice President and Chief Operating Officer
Battelle Memorial Institute

As Executive Vice President and Chief Operating Officer of the world's largest independent research and development organization, Dr. Olesen oversees Battelle's five major operating divisions in the United States and Europe. Battelle's 7,500 staff members annually develop new products and processes that some 2,000 worldwide industrial clients commercialize or incorporate into their own manufacturing or business operations. Battelle also serves government agencies through managing large programs and providing technical services and systems analyses. Annual revenues approach $500 million.

Since being named to his present post in 1984, Dr. Olesen has restructured Battelle to bring the organization into new areas of technology that meet shifting demands of its client base. Known as an aggressive business leader, Dr. Olesen has implemented strategies to penetrate into new professional services and create an entrepreneurial environment for performing quality work.

Major business areas of Battelle are: biotechnology, chemistry, electronics, energy, engineering systems, environmental sciences, information processing, manufacturing, materials, national security, and transportation systems. Clients represent 40 countries and include many Fortune 500 companies, medium- and small-sized manufacturers, associations, utilities, service organizations, and all levels of government agencies. Operating divisions are located in Columbus, Ohio; Richland, Washington; Geneva, Switzerland; and Frankfurt, Germany.

Dr. Olesen began his career at Battelle in 1967, and has progressed through a series of senior management positions, including directing Battelle's Pacific Northwest Division in Richland, Washington. His technical area of expertise is in water and waste management.

DISCUSSION

Mr. **Slaughter**. Thank you, Dr. Bagley.

Do you have any opinion on the optimum relationship between Federal support for research done in academic institutions as compared to industry?

Dr. **Bagley**. I believe there's certainly a level of support that's most useful to be conducted in academic institutions, and I would say probably that in the past that level has not been at a level where it should be. But in terms of what the percentage of the total Federal investment should be in academic institutions, I don't think there's a magic answer. It probably depends on the specific field. But to be specific, probably the trend of more research being put in universities is a good trend.

Mr. **Slaughter**. Do you think that the Federal Government should directly participate in more research than it does now, or do you think the policy has been—the balance has been about right?

Dr. **Bagley**. I think over time, probably for a variety of reasons, the most significant being availability of Federal dollars, the level has not been what it should have been, and we may be seeing the impact of that now in terms of competitiveness in our economy.

But the trend of the last few years of more Federal support for research is obviously a good sign, and I think we must—both Congress and the executive branch—ensure that there not be any decline again from where we are in terms of percentage of GNP or percentage of Federal expenditures.

Mr. **Slaughter**. Do you have any suggestions about possibly tying the level of Federal expenditures for research support to GNP or any measure like that?

Dr. **Bagley**. That's a tough question to answer. I think that's one—that's been one criterion in the past. I think that should be one criterion we ought to look at. But I'm not so sure, given the total demand on Federal resources, that it be desirable to say we must meet a certain percentage of GNP. I think there's a range that's probably a desirable range, but without getting into a specific formula which may be difficult to——

Mr. **Slaughter**. And you might not have a steady level of support either that most people think the Federal Government should give to research.

Dr. **Bagley**. Well, I think it's more important to have a steady level of support, without radical changes or significant swings, than to say we must achieve a certain percentage of GNP in order to remain competitive. I think that's a desirable objective. But the key is stability.

Mr. **Slaughter**. Well, thank you very much, Dr. Bagley.

This concludes our hearing for today, and the task force stands adjourned until tomorrow at 10 o'clock, and we will meet in room 2318 for the second day of hearings on research funding as an investment.

[Whereupon, at 12 noon, the task force was adjourned, to reconvene the following day, April 30, 1986, at 10 a.m.]

RESEARCH FUNDING AS AN INVESTMENT

WEDNESDAY, APRIL 30, 1986

House of Representatives,
Committee on Science and Technology,
Science Policy Task Force,
Washington, DC.

The task force met, pursuant to call, at 10:07 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the task force) presiding.

Mr. Fuqua. This is the second day of hearings by the Science Policy Task Force on "Research Funding as an Investment." Yesterday the task force heard testimony from the Office of Technology Assessment, which presented a technical memorandum on the subject, witnesses who commented on past and present practices in R&D transfer methodology, and the viewpoints from the private research and investment banking communities.

The series continues today with speakers renowned in the fields of economics, corporate management and research, as viewed by private industry. Our first witness is Dr. Zvi Griliches, chairman of the Department of Economics at Harvard University, who is noted for his work in economic returns for research and development.

The second view of economics is a panel comprised of Dr. Ali Seireg and Dr. Wassily Leontief, who are collaborating on a project sponsored by the American Society of Mechanical Engineers aimed at quantifying the impact of science and technology on industrial productivity and international competitiveness. Dr. Seireg is Ebough professor of mechanical engineering at a place that I spent some time, the University of Florida, and experimented in the field of machines and mechanisms and mechanical systems. Dr. Leontief is director of the Institute of Economic Analysis at New York University and a 1973 recipient of the Nobel Prize in economics for his work on input-output techniques. We could probably use that around Congress sometimes. He may get another prize if he can devise a method on that.

The third presentation will be by Dr. Richard Foster, director of McKinsey & Co., a management consultant, whose field is specialization in the problems of managing technology.

The concluding witness is Dr. Lamont Eltinge, director of research for the Eaton Corp., and a former member of the Industrial Research Institute, who will address the question of research funding from an industrial perspective.

Dr. Griliches, we would be pleased to hear from you at this time.

STATEMENT OF DR. ZVI GRILICHES, NATIONAL ROPES PROFESSOR OF POLITICAL ECONOMY, HARVARD UNIVERSITY, AND DIRECTOR, PROGRAM ON PRODUCTIVITY AND TECHNICAL CHANGE STUDIES, NATIONAL BUREAU OF ECONOMIC RESEARCH, CAMBRIDGE, MA

Dr. Griliches. Thank you, Mr. Chairman. I am glad to be back before this committee.

In my previous appearance before this panel almost a year ago, I discussed the contribution of basic research to productivity growth based on a study of almost 1,000 of the largest corporations in U.S. manufacturing, which has since been published, and I alluded to the difficulties involved in evaluating the contribution of Federal R&D expenditures to productivity growth. Since today's topic is research funding as an investment, I want to return, in part, to some of my earlier remarks and discuss the difficulties that are involved in measuring the returns to such an investment, and then trying to derive useful conclusions from data about it. In addition to outlining the difficulties that beset one when trying to evaluate the Federal R&D effort, I will also comment briefly on the OTA study prepared for this committee, agreeing with its main conclusions but disagreeing with the tone of some of its implications and return to my earlier complaints about the state of the data and research support in this area.

There is no doubt in my mind that Federal expenditures in R&D have made a major contribution to economic growth in this country, and that has been documented in some of my own work, and in the work of many other economists. I think the difficulty in evaluating the impact of publicly-funded R&D expenditures on aggregate economic activity, using the standard economic data, is that we are looking for something that is largely not even there—for reasons having to do both with the nature of much of R&D and the nature of our GNP and productivity accounts.

There are at least two problems here. First, most of the public R&D consists of research on defense and space exploration for which no adequate market valuation exists. By accounting convention, outputs of this research are measured by costs, resulting in zero contribution to measured productivity. For example, whether a shuttle launch is successful or not, the effect on measured GNP is the same. The contribution of the space effort is measured by the inputs into it, the cost of man-hours and hardware, and not by the success of these efforts. Similarly, whether one missile is better than what it is supposed to replace and whether our security situation has improved or deteriorated as the result of research expenditures on military technology will not show up in the growth of national productivity as it is currently measured.

The same argument can be made about the very important public research expenditures on health that lead to increased life expectancy and a lower incidence of some diseases. These also would not show up in the current productivity accounts. Currently, such an improvement in health would raise both measured GNP, and the hours worked by the labor force, leaving productivity—output per manhour—largely unchanged. The contribution of such expenditures could be computed if we evaluated the changes in the productivity of obtaining hours of work from a given population. But that kind of calculation is not within the framework of the national income accounts as they are currently constructed.

Second, much of the product of private R&D investments is in the form of new commodities or improvements in the qualities of old commodities. Whether this product shows up in real GNP, in constant prices, depends on what happens to the price indices with which the current output of the particular industry is deflated. If the price indices were to recognize these improvements in quality fully, the resulting GNP measures would reflect the social product of this research. But that is unlikely. Since price indexes fail to reflect these quality improvements, only the private product will show up in the real accounts, to the extent that firms succeeded in appropriating it via higher prices for newer higher quality products.

Examples of such things which are not measured in the national income accounts are the whole improvements in the technology of calculating—from pocket calculators to PC's—and other timesavers such as the air transport industry. The fact that one can get to the west coast to the east coast in half the time or less required several decades ago leaves hardly a trace in the national productivity accounts.

In an earlier paper I tried to guess the extent of this problem. By looking in some detail at the sources of funds and the announced uses of them, I concluded that in 1970 only about 40 percent of the total R&D expenditures could be thought as likely to leave traces of their direct contribution in the national accounts. Today, I would think the fraction is even lower, probably on the order of a quarter or so. This is also implied by Mr. Bloch's current article in Science.

This brings me to my last point: The current lack of appreciation and support for data collection and research in this area. But before I do that, I would like to comment briefly on the OTA study, "Research Funding as an Investment: Can We Measure the Returns?" which covers, in part, similar ground.

The main conclusion of the OTA study is a negative answer to your question—its title—and by implication, a rebuttal of the notion that we have currently the tools or the data to put Federal R&D management on a scientific footing. In this, I am in agreement with the study. In fact, I think it's fair to say that significant parts of the study are based on my own work and have quoted and summarized it fairly. What I do find bothersome, somewhat, is the implication that is left in this report that economics has nothing to contribute here, that the economic framework has somehow failed to deliver. We did not promise you a rose garden. Nobody said that one can use economic tools to answer all difficult policy questions. The tools are just tools. They require appropriate ingredients or materials to work with—a point I will come back to below. What bothers me is the implication that if economics does not have the answer, then you may just ignore it. In favor of what: seat of the pants hunches or political logrolling?

Economics is more than just a particular numerical formula or a set of instructions for computing a rate of return on investment. It is a framework for coherent thinking about the costs and benefits of choices in an uncertain and limited resources world. It is a dismal science, in the sense that it reminds us that a dollar spent on AIDS research could have been spent on research on Alzheimer's disease, on retraining workers who lost their jobs due to plant closings, or on school lunches and/or birth defect prevention. That we do not know how to make such choices well does not mean that we should not try. The alternative to it is the current system where much is decided by who has the louder voice or the more effective Congressman.

Economics provides a way of arguing about such choices. It indicates the kind of data that you would need and the orders of magnitude that would have to prevail to convince one that one choice is significantly better than another. It imposes some discipline on the discussion. Not all hunches and assertions are equally credible. The imposition of the requirement of benefit-cost computations on the Army Corps of Engineers' water projects did not eliminate all of the pork from these projects, but it did raise the discussion to a new level. The fact that we do not have a clean and effective framework for allocating R&D expenditures does not mean that questions of relative benefits and costs should not be put forward. There may not be good answers to such questions, but the process of conversing about choices in economic terms will, by itself, elevate and discipline the discussion about them. What is the alternative? Thus, I do not think that economics failed here. It is still the best way of thinking about such problems, though actual choices will require more detailed information about the issues under consideration and, ultimately, a leap of faith.

Making investments in private or public R&D is an entrepreneurial decision. It is a gamble in an uncertain world. It can be better informed, based on a more extensive quantitative base and theoretical framework, but it cannot assure the right answer in advance. Research is exploring the unknown and one cannot know or prove that one way of doing that is necessarily better than another. Nevertheless, thinking it through is likely to be better than not doing so, and that is what economics is about.

I do want to add here a comment on the research as an investment metaphor. Indeed, research is an investment, in the sense that it takes time to mature and that its results are highly uncertain. But it is more like a fruit tree than a building or a piece of equipment, which can be bought or constructed and then lasts a long time. The research process is interdependent over time and is embodied in perishable people. You cannot plant a tree and stop watering it, or pull it up and replant it every few years, and expect a good yield of fruit from it. Long-term commitments, steadfastness and luck, luck in the choice of people and questions, are the main ingredients of a successful research program.

I would like to return now to my worry about the state of the empirical base for such decisions. Because research output is rarely sold in the market or observed directly, it is rather difficult to collect and analyze the relevant statistics about it. We would like to know who does it, to what extent, and in which direction. The Bureau of the Census collects R&D data from companies on behalf of the NSF. These data are imperfect in part because companies now are so variegated, and because not enough detail is collected on what companies are actually spending their R&D moneys on.

The whole effort is also quite underfunded. The unit at the Census dealing with these data is small and overextended, and because of budget cuts the collection of the detail on basic versus total R&D split, and on the allocation to product fields is being done only in alternative years with very little effort devoted to studying how these measures could be improved.

On the output side, one of the few quantitative indicators of inventive activity available to us are patent counts, and the more detailed information contained in the patent documents themselves. Over the last decade a small office within the Patent Office, the Office of Technology Assessment and Forecast, had struggled valiantly to make some of these data available to researchers and analysts. Data are not usable in raw form. They have to be organized, computer programs have to be designed and written, and access procedures have to be developed. OTAF has made possible the study of patent statistics in the modern age and provided tools for tracking what is happening in specific technologies and to the U.S. competitive position in them. Last year it was abolished as part of an economy move at the Patent Office and the overall drive by this administration to reduce the amount of data collected and made available by various Government agencies. The notion that private-sector information firms will step in and take over this role ignores the public-good aspect of information as a product, and the large economies of scale in developing data management and data access techniques. It seems that this administration not only does not want to know the consequences of its various economic policies, but is also trying to make it difficult for others to find out.

The committee's agenda for a study of Government science policy is full of interesting and important questions. One of the questions it does not ask, however, is "Where will the answers come from?" Where is the factual base and the scientific expertise to be found to answer such questions? One of the few units in the Federal Government that saw as its mission to study such questions and to support research targeted at trying to produce answers to your agenda, the Policy Research Analysis Unit of the NSF, also had its budget and role cut significantly. If these questions are important, if we want to understand more about our economy, about science and technology, and about their impact on industry and on our lives in general, we will need to figure out ways of encouraging and supporting the study of such questions and the collection of data on the relevant aspects of it. If the decisions are to be better informed, the relevant information has to be collected, processed and analyzed, and the whole endeavor has somehow to be financed.

In economics we say there is no such thing as a free lunch. My guess is that there is not one in policy analysis and public decisionmaking, either.

Thank you.

[The prepared statement of Dr. Griliches follows:]

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ZVI GRILICHES
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April 30, 1986

Testimony of ZVI GRILICHES before the Task Force of the Committee on Science and Technology. Hearing on "Research Funding as an Investment."

My name is Zvi Griliches. I am Nathaniel Ropes Professor of Political Economy at Harvard University and Director of the Program on Productivity and Technical Change Studies at the National Bureau of Economic Research. I am also a member of the National Academy of Sciences Committee on Science, Engineering and Public Policy. I have spent much of my professional life studying technical change and the role of R&D expenditures in it and I have recently completed a study of the productivity growth of major U.S. corporations in the 1970s with a special emphasis on the role of basic research in it.

In my previous appearance before this panel a year ago (April 24, 1985), I discussed the contribution of basic research to productivity growth, based on a study of data for almost one thousand of the largest corporations in United States manufacturing which has since been published (American Economic Review, March 1986) and alluded to the difficulties involved in evaluating the contribution of Federal R&D expenditures to productivity growth and economic welfare in general. Since the last topic is related to your deliberations today, I cannot do better than reproduce (in slightly altered form) some of my earlier remarks. In addition to outlining the difficulties that beset one when trying to evaluate the Federal R&D effort, I will also comment briefly on the OTA study prepared for this committee, agreeing with its main conclusions but disagreeing with the tone of some of its implications, and return to my earlier complaints about the state of the data and research support in this area itself, the effort to understand what R&D is and does, how it is generated, what its consequences are, and how one may wish to intervene, if at all, in order to improve its outcomes.

In looking for the effects of publicly funded R&D expenditures on aggregate economic activity using various productivity measures we may be looking for something that is largely not even there -- for reasons having to do both with the nature of much of R&D and the nature of our GNP and productivity accounts. There are at least two problems here: First, most of the R&D "product" is sold to the public sector. It consists of research on defense and space exploration, for which no adequate market valuation exists. By accounting convention, these "outputs" are measured by costs, resulting in zero contribution to measured productivity, by definition. (This is true for the bulk of defense and space research, which is contract research with an end-product of blueprints, formulae, or prototypes. It is also largely true for the research components of the "hardware" sold to the government, because of the lack of appropriate price indexes by which to deflate such expenditures.) For example, whether the moon landing was successful or not, the effect on measured GNP would have been the same. The contribution of the space effort is measured by the inputs into it, the cost of man-hours and hardware, and not by the success of these efforts. Similarly whether the MX missile is better than what it is supposed to replace and whether our security situation has improved or deteriorated as the result of research expenditures on military technology will not show up in the growth of national productivity as it is currently measured.

Or, taking another example, consider public research expenditures on health that lead to increased life expectancy and a lower incidence of some diseases. These also would not show up in the current productivity accounts. Currently, such an improvement would raise both measured GNP and the hours worked by the labor force, leaving "productivity" (output per man-hour) largely unchanged. The contribution of such expenditures could be computed if we evaluated the changes in the "productivity" of obtaining hours of work from a given population. But that kind of calculation is not within the framework of the national-income accounts as they are currently constructed.

Second, much of the product of private R&D investments is in the form of new commodities or improvements in the qualities of old commodities. Whether this product shows up in "real" GNP (in constant prices) depends on what happens to the price indexes with which the current net output of the particular industry is deflated. If the price indexes were to recognize these improvements in "quality" fully, the resulting real-GNP measures would reflect the social product of this research. But that is unlikely. If the price indexes fail to reflect these quality improvements, only the private product will show up in the real accounts -- to the extent that firms succeeded in appropriating it via higher prices for the newer higher quality products.

If some of these unmeasured improvements are attached to products used in turn as inputs into the production of other private products, their contribution will show up in the productivity measures of the industries that purchased them. Thus, for example, though the contribution of research on improving the performance of farm tractors may not show up in the output-input account of the tractor industry, it will have an effect on measured productivity in agriculture. But many new products are sold directly to consumers, and the improvements associated with their use are not caught in the conventional measures of GNP or consumption per capita. Examples are time-savers like pocket calculators or the whole air-transport industry: The fact that one can get to the West Coast from the East Coast in half the time required 25 years ago leaves hardly a trace in the national productivity accounts.

In an earlier paper ("Research Expenditures and Growth Accounting," 1973) I tried to guess the extent of this problem. By looking in some detail at the sources of funds and the announced uses of them, I concluded that in 1970, out of the $27 billion or so identified as "research and development" expenditures, half could be attributed to defense and space activities. Of the remainder, about $11 billion could be thought of as having a potential impact on aggregate productivity. These $11 billion consisted of $9.5 billion spent in industrial research; about $0.3 billion in intramural Federal research; and $1.3 billion in research in universities, institutes, and similar organizations. More recent data do not indicate any significant change in the allocation of R&D expenditures. Significantly less than a half of the total R&D expenditures in the economy are likely to leave traces of their direct contribution in the national productivity accounts. This does not mean that these expenditures are necessarily unproductive, only that we have no good way of measuring their effects. This brings me to my last point: The current lack of appreciation and support for data collection and research in this area. But before I do that, I would like to comment briefly on the OTA study ("Research Funding as an Investment: Can We Measure the Returns?", 1986), which covers, in part, similar ground.

The main conclusion of the OTA study is a negative answer to your question (its title) and by implication, a rebuttal of the notion that we have currently the tools or the data to put Federal R&D management on a "scientific" footing. In this, I am in agreement with the study. In fact, I think that it is fair to say that significant parts of this study are based on my own work and have quoted and summarized it fairly. What I do find bothersome, somewhat, is the implication that is left that economics has nothing to contribute here, that the economic framework has somehow "failed" to deliver. We "did not promise you a rose garden"! Nobody said that one can use economic tools to answer all difficult policy questions. The tools are just tools, they require appropriate ingredients or materials to work with (a point that I will come back to below). What bothers me is the implication that if economics does not have the answer than you may just ignore it. In favor of what? "Seat of the pants" hunches or political logrolling? Economics is more than just a particular numerical formula or a set of instructions for computing a rate of return on investment. It is a framework for coherent thinking about the costs and benefits of choices in an uncertain and limited resources world. It is a "dismal science," in the sense that it reminds us that a dollar spent on AIDS research could have been spent on research on Alzheimers disease, on retraining workers who lost their jobs due to plant closings as the results of advances in Japanese technology and the high value of the dollar, on school lunches and/or birth-defects prevention. That we do not know how to make such choices well does not mean that we should not try. The alternative to it is the current system where much is decided by who has the louder voice or the more effective Congressman.

Economics provides a way of arguing about such choices, it indicates the kind of data that you would need and the orders of magnitude that would have to prevail to convince one that one choice is significantly better than another. It imposes some discipline on the discussion. Not all hunches and assertions are equally credible. The imposition of the requirement of benefit-cost computations on the Army Corps of Engineers water projects did not eliminate all of the pork from these projects but did raise the discussion to a new level and eliminate some of the most egregious boondogles. The fact that we do not have a clean and effective framework for allocating R&D expenditures does not mean that questions of relative benefits and costs should not be put forward. They may not have good answers, but the process of conversing about the choices in such terms will by itself elevate and discipline the discussion about them. What is the alternative? Thus, I do not think that economics "failed" here. It is still the best way of thinking about such problems though actual choices require much more detailed information about the issues under consideration and, ultimately, a leap of faith. Making investments in private or public R&D, is an enterpreneurial decision. It is a gamble in an uncertain world. It can be better informed, based on a more extensive quantitative base and theoretical framework, but it cannot assure the "right" answer in advance. Research is exploring the unknown and one cannot know or prove that one way of doing that is necessarily better than another. Nevertheless, thinking it through is likely to be better than not doing so, and that is what economics is about.

I would like to return now to my worry about the state of the empirical base for such decisions. Because research "output" is rarely sold in the market or observed directly, it is rather difficult to collect and analyze the relevant statistics about it. We would like to know who does it, to what extent, and in which direction. The Bureau of the Census collects R&D data from companies on behalf of the NSF. These data are imperfect in part because companies now are so variegated and because not enough detail is collected on what companies are actually spending their R&D monies. The whole effort is also quite underfunded. The unit at the Census dealing with these data is small and overextended and because of budget cuts the collection of the detail on basic vs. total R&D split and on the allocation to product fields is being done only in alternative years with very little effort devoted to studying how these measures could be improved.

On the output side, one of the few quantitative indicators of inventive activity available to us are patent counts and the more detailed information contained in the patent documents themselves. Over the last decade a small office within the Patent Office, the Office of Technology Assessment and Forecast, has struggled valiantly to make some of these data available to researchers and analysts. Data are not usable in raw form. They have to be organized, computer programs have to be designed and written, and access procedures have to be developed. OTAF has made possible the study of patent statistics in the modern age and provided tools for tracking what is happening in specific technologies and to the U.S. competitive position in them. Last year it was abolished as part of an economy move at the Patent Office (another example of penny-wise and pound-foolish) and the overall drive by this administration to reduce the amount of data collected and made available by various government agencies. The notion that private sector information firms will step in and take over this role ignores the public good aspect of information as a product and the large economies of scale in developing data management and data access techniques. It seems that this administration not only does not want to know the consequences of its various economic policies but is also trying to make it difficult for others to find out.

The Committee's "Agenda for a study of government science policy" is full of interesting and important questions. One of the questions it does not ask, however, is "Where will the answers come from?". Where is the factual base and the scientific expertise to be found to answer such questions? One of the few units in the Federal government that saw as its mission to study such questions and to support research targeted at trying to produce answers to your "Agenda," the Policy Research Analysis unit of the NSF, also had its budget and role cut significantly. If these questions are important, if we want to understand more about our economy, about science and technology, and about their impact on industry and on our lives in general, we will need to figure out ways of encouraging and supporting the study of such questions and the collection of data on the relevant aspects of it. If the decisions are to be better informed, the relevant information has to be collected, processed and analyzed, and the whole endeavour has somehow to be financed. We say, in economics, that there is no such thing as a free lunch. My guess is that there is not one in policy analysis and public decision making either.

Productivity, *R&D*, and Basic Research at the Firm Level in the 1970's

By ZVI GRILICHES[*]

This paper reports new results on the relationship of research and development (*R&D*) expenditures, especially expenditures on basic research, to productivity growth in U.S. manufacturing firms during the 1970's. It is based on a unique data set, the National Science Foundation (NSF) *R&D*-Census match, containing information on *R&D* expenditures, sales, employment, and other detail for approximately 1000 largest manufacturing firms from 1957 through 1977. It updates my earlier work (1980) on the precursor of this data set, replicates some of Edwin Mansfield's (1980) work on the contribution of basic research to productivity growth using a larger, more recent, and more representative sample of firms, and complements similar work by myself and J. Mairesse (1983,1984) based on a publicly accessible but more limited data set.

Two topics are explored in some detail: 1) Is there any evidence of a decline in the returns to industrial *R&D* expenditures, a decline in their "fecundity" in the 1970's as compared to earlier time periods? 2) Is there evidence that basic research is a relatively more important component of *R&D* and that there may have been an underinvestment in this component?

A few background facts are worth stressing at this point. In the United States, total *R&D* expenditures in industry peaked (in real terms) around 1968, dropped slightly in the early 1970's and recovered somewhat in the late 1970's. Relative to total sales, *R&D* expenditures in industry declined from 4.2 percent in 1968 to a trough of 2.6 percent in 1979 and then recovered to 3.7 percent by 1982. This pattern masks a strong divergence between the trends in federally and privately supported industrial *R&D*. Federally supported *R&D* fell from 2.1 percent of manufacturing sales in 1967 to 0.7 percent in 1979 and has only recently begun to recover, while company-financed *R&D* stayed essentially constant (relative to industry sales) with almost all of the fluctuation coming from the decline in federal support (NSF, 1983; 1984). During the same period, the economy experienced one of the sharpest and prolonged recessions of the postwar period and a large and pervasive productivity slowdown. Hardest hit were the primary metals, motor vehicles, and other heavy, energy-related industries. On the whole, these were the less *R&D* intensive industries, resulting in a largely accidental correlation between *R&D* intensiveness and the productivity slowdown. (See my 1980 papers and my article with F. Lichtenberg, 1984, for more discussion of these issues.)

The remainder of the paper is organized as follows. First, I describe the data set with its advantages and limitations and present some overall comparative statistics. Second, I outline briefly the framework that underlies the computations to be performed. The results are presented and discussed, and the paper closes with some conclusions, caveats, and suggestions for further research.

I. Previous Work and the Current Data Set

The current project is an extension of work originally begun in the mid-1960's. That work was based on the matching of *R&D* data collected on behalf of the NSF by the Bureau of the Census during 1957–65 with additional company data from the 1958 and 1963 Census of Manufactures and Enterprise Statistics. The universe consisted of large (1000 or more employees) U.S. manufacturing companies performing *R&D*. The final sample of 883 of such companies accounted for over 90 percent of total sales and *R&D* expenditures of all firms in this universe.

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The main finding of that work (see my 1980a paper) was a rather consistent and positive relationship between various measures of company productivity and its investments in research and development. The Cobb-Douglas-type production functions, estimated on both levels (1963) and rates of growth (1957–65) yielded an elasticity of output with respect to *R&D* investments of about .07 and an implied average gross excess rate of return of 27 percent (as of 1963), a significantly lower rate of return to federally financed *R&D* expenditures, and no clear evidence of significant scale effects either in *R&D* investment policies or the returns from it.

In trying to extend the earlier study to the more recent time period, it became clear that the earlier work could not be simply updated because much of the earlier data was lost and a new data set had to be created instead. The basic objective was to create a matched body of data on most of the large *R&D* performing corporations in the United States, making it possible to analyze both the determinants and consequences of *R&D* spending *over time*. For this purpose a time-series record has been created for each company consisting of the major variables in the annual *R&D* survey for each of the years 1957–77, supplementary *R&D* information for selected years (1962, 1967, 1972, and 1975), data from the Enterprise Statistics (i.e., company level questionnaires) for 1967, 1972, and 1977, and a few additional items from the Census of Manufactures establishment record summaries for 1967 and 1972. The data set began with all the "certainty" companies in the NSF *R&D* survey as they existed in 1972. There were approximately 1100 such companies, but a "complete" record is available only for a much smaller number.[1]

Table 1 lists the sample size, means, and standard deviation for the major variables as of 1972 and their growth rates from 1966 to 1977. It is intended to describe three aspects of these data: 1) the general characteristics (means and standard deviations) of the sample as of 1972; 2) average rates of growth of the major variables of interest during the 1967–77 period; and 3) how these measures change when the sample is changed to select observations according to the availability of the requisite information.

Turning to the last topic first, note that we tend to lose smaller and more *R&D* intensive firms as the sample gets more restrictive. The first column of Table 1 corresponds to the most liberal criterion: a firm had to exist in 1972 and report positive *R&D*.. Column 2 requires both the ability to compute a growth rate for the 1967–77 period (i.e., at least five good time-series observations) and a successful match to the 1972 Census of Enterprise data (NCK–1). In column 3, I add the requirement of a successful match to the 1977 Census data, while in column 4 the subsample is based on a match with the 1967 and 1972 Census data instead. The major differences occur in the transition from column 1 to column 2 where trying to match to the Census we lose a relatively large number of smaller firms for which there are still data in the *R&D* survey files. The firms that can be also found in the 1977 Census are slightly larger and have had a somewhat higher rate of growth in employment, *R&D*, and productivity. The firms that also existed in 1967

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[1] The universe of this data match consists of all "certainty" cases in the 1972 *R&D* survey; i.e., the basic definition is the population of companies as they existed in 1972 (as against 1962 in the earlier study) and the requirement of "certainty" assures that the Census Bureau tried to collect consistent data for these firms for more than one year. The "certainty" cases correspond closely to the earlier restriction to companies with 1000 or more employees, though it is a bit more inclusive. See my paper with Bronwyn Hall (1982) and Hall (1984) for more detail on sample definition and variable construction.

Table 1—Major Variables in 1972 and 1966-77 Growth Rates by Subsample: Means and Standard Deviations[a]

Variable	Data Set, Selection Criteria, and Sample Size 1972 *R&D* Survey Universe ($N = 1105$) (1)	1966–77 Growth Rates Computable and Matched to 1972 Census ($N = 652$) (2)	(2) and Matched to 1977 Census ($N = 491$) (3)	(2) and Matched to 1967 Census ($N = 386$) (4)
A. Levels in 1972				
Sales in Million Dollars	146 (1.61)	205 (1.43)	223 (1.40)	236 (1.44)
Total Employment	4038 (1.48)	5570 (1.27)	6212 (1.30)	6698 (1.31)
R&D Scientists and Engineers	89 (1.66)	74 (1.70)	82 (1.71)	106 (1.72)
R&D in Million Dollars	2.3 (1.74)	3.0 (1.78)	3.4 (1.77)	4.3 (1.83)
R&D to Sales Ratio (*RS*)	.051 (.131)	.033 (.064)	.032 (.051)	.035 (.048)
Company *R&D*/Sales Ratio (*CRS*)	.028 (.069)	.022 (.026)	.023 (.026)	.025 (.026)
Basic to Total *R&D* Ratio (*BR*)	.025 (.074)	.026 (.071)	.026 (.075)	.027 (.073)
Value-Added, Million Dollars		100 (1.32)	113 (1.31)	121 (1.34)
Gross Fixed Assets Million Dollars		115 (1.67)	124 (1.59)	147 (1.65)
B. Growth Rates 1966–77				
Employment Growth		.012 (.046)	.015 (.041)	.006 (.040)
Partial Productivity Growth (*BPT*)		.025 (.036)	0.26 (.034)	.025 (.035)
Total *R&D* Growth, Deflated (*BTRD*)		−.001 (.079)	.003 (.074)	−.007 (.070)
Scientists and Engineers Growth		.008 (.087)	.012 (.084)	.004 (.078)
Company *R&D* Growth, Deflated (*BCRD*)		.004 (.081)	.008 (.076)	−.000 (.071)

Notes: Col. 1: "Certainty" firms in the NSF *R&D* Survey with positive *R&D* in 1972; Col. 2: Growth rates for 1966–77 computable (at least 5 years of good data on sales, employment, and *R&D*) and a successful match to the 1972 Enterprise Census (NCK-1); Col. 3: (2) and a successful match to the 1977 Census (NCK-1); Col. 4: (2) and a successful match to the 1967 Census and growth rates computable for 1957–65; Partial productivity growth = deflated sales growth − (share of labor compensation in total sales) × growth in employment; Sales deflated by NIPA based output price indexes at the 2–3 digit SIC level. *R&D* deflator based on the methodology suggested by Jaffe (NSF), from my 1984 comment.

[a] Geometric means and standard deviations (shown in parenthesis) of the logarithms (approximate coefficient of variation) except for growth rates or ratios.

are even larger but have on average grown somewhat more slowly than those that existed in the 1972–77 period. If we look at two of the major variables of interest, partial productivity growth and the ratio of basic to total *R&D*, there is almost no difference in their means across the relevant columns (2, 3, and 4), and hence it is unlikely that subsequent conclusions will be subject to a serious sample selection bias. I will, therefore, ignore this topic here.

Looking at the levels of the variables in 1972, we see that the average firm in the sample is quite large (5000+ employees), employs close to one hundred *R&D* scientists and engineers, and is making only a relatively modest investment of its own money (about 2.5 percent of sales) in *R&D*, with very little of that, less than 3 percent, being devoted to basic *R&D*. This picture is somewhat misleading, however. The actual distribution of firms is quite skewed, with a small number of larger firms spending much larger amounts on both total and basic *R&D*. Looking at growth rates one can observe that on average these firms grew only moderately during this period: about 1 percent per year in total employment, about 2.5 percent per year in partial productivity, and almost zero growth in deflated *R&D* expenditures (though a slightly positive rate of growth in the number of *R&D* scientists and engineers). Here again, while on average there is little movement, there is a great deal of variability at the individual firm level. The standard deviations of the rates of growth of partial productivity and total *R&D* are 3.5 and 8 percent per annum, respectively, with many firms growing much faster (and also much slower) than the average.

Looking at some of the *R&D* ratios over time, not reported in Table 1, one cannot see any significant decline in the rate of private investment in *R&D*. While the total *R&D* to sales ratio falls from .042 in 1962 to .035 in 1972 and again from .032 in 1972 to .029 in 1977 for firms in subsamples 4 and 3, respectively, the company-financed *R&D* to sales ratios (*CRS*) are essentially unchanged (.025 in 1962 and 1972 in subsample 4 and .023 in 1972 and 1977 in subsample 3). On the other hand, while the basic research ratio (*BR*) fell only modestly from .033 to .031 between 1962 and 1972, and from .027 to .023 between 1972 and 1977, coupled with the decline in the overall total *R&D* to sales ratio, this implies about a 40 percent reduction in the relative intensity of industrial investment in basic research, relative to industry sales. Almost all of this decline came from the overall decline in federally financed *R&D* which declined from about 55 percent of total *R&D* in industry in 1965 to about 35 percent in 1982. The federal government financed about 32 percent of all basic research in industry in 1967 but only 19 percent in 1982 (see NSF, 83-302 and 84-311). The reduction was so steep that basic research in industry declined not only relatively (to sales) but also absolutely, from a peak of $813 million in 1966 (in 1977 dollars) to a trough of $581 million in 1975 and did not surpass the 1960's levels until the early 1980's. How one interprets the consequences of such a decline depends on one's view of the relative productivity of governmentally financed *R&D* expenditures in industry, a topic I will be exploring below.

II. The Analytical Framework and Econometric Results

The work reported here focuses primarily on the analysis of productivity growth for these companies, using a rather simple Cobb-Douglas production function approach:

$$(1) \qquad Q_t = Ae^{\lambda t}K_t^{\alpha}C_t^{\beta}L_t^{1-\beta},$$

where Q is output (sales, or value-added), C and L are measures of capital and labor input, respectively, $K = \sum_i w_i R_{t-i}$ is a measure of the accumulated and still productive research capital ("knowledge"), R_t measures the real (deflated) gross investment in research in period t, and the w_i's connect the levels of past research to the current state of knowledge. In addition, λ measures the rate of disembodied "external" technical change (where t is time in years), A is a constant, and constant returns to scale have been assumed with respect to the conventional inputs (C and L).

A number of serious difficulties arise when one turns to the operational construction of the various variables (see my 1979 article for more detailed discussion). Perhaps the two most important problems are the measurement of output (Q) in a research-intensive industry (where quality changes may be rampant), and the construction of the unobservable research capital measure (K). Turning to the second problem first, note that $K_t = \sum w_i R_{t-i}$ can be thought of as a measure of the distributed lag effect of past research investments on productivity. There are at least three forces at work here: the lag between investment in research and the actual invention of a new technique or product; the lag between invention and the development and complete market acceptance of the new product; and its disappearance from the currently utilized stock of knowledge due to changes in external circumstances and the development of superior techniques or products by competitors (depreciation and obsolescence). There is some scattered evidence, based largely on questionnaire studies that such lags are rather short in industry, where most of research expenditures are spent on development and applied topics, and where the private returns from *R&D* become obsolete much faster due to the erosion of a firm's specific monopoly position (Ariel Pakes and M. Schankerman, 1984).

While my models are written as if the main point of research expenditures is to increase the physical productivity of the firm's production process, most of the actual research in industry is devoted to the development of new products or processes to be sold and used outside the firm in question. Assuming that, on average, the outside world pays for these products what they are worth to it, using sales or value-added as the dependent variable does, in fact, capture the private returns to such research endeavours. However, the observed private returns may underestimate the social returns because, given the competitive structure of the particular industry, the firm is unlikely to appropriate all of these returns. On the other hand, part of the increase in the revenues of a particular firm may come at the expense of other firms, or from changes in the market power induced by the success of its research program. I cannot say much about the net impact of such forces on the basis of the data at hand. This would require a detailed comparison of the individual firm results with estimates based on industry and economy-wide returns to research, a topic beyond the scope of this project. But since expected private returns are a determinant of private investment flows into this activity, they are of some interest even if one cannot answer the social returns question unequivocally.

This framework can be extended to ask whether different types of *R&D* (private vs. federal, or basic vs. applied) are equally "potent" in generating productivity growth. One way of answering this question is to look at the "mix" of *R&D* expenditures and ask if it matters for the question at hand. Let there be two types of *R&D* expenditures, R_1 and R_2, and let us assume that the overall analysis is in terms of the logarithm of total *R&D* expenditures but that we believe that R_2 should have been weighted more, given a δ premium (or discount). That is, the right variable is

$$R^* = R_1 + (1+\delta)R_2 = R(1+\delta s), \tag{2}$$

where $s = R_2/R$ is the "share" of R_2 in total $R = R_1 + R_2$. Then the $\alpha \log R^*$ term can be approximated by $\alpha \log R^* \approx \alpha \log R + \alpha\delta s$. The sign and significance of the mix term s will give us some clue about the size and magnitude of the δ term.

A similar argument can be made also in the context of a growth-rate formulation. Let lower case letters denote growth rates. Then $r = (1-s)r_1 + sr_2$ while $r^* = (1-s)r_1 + (1+\delta)sr_2$. If, as is mostly the case in our data, the growth rates of r_1 and r_2 are roughly equal, then $r^* = r(1+\delta s)$, and again, the coefficient of the mix term s provides us with some information about the "premium" or "discount" on R_2 since αr^* can be approximated by

$$\alpha r^* \approx (\alpha + \delta\bar{s})r + (\alpha\bar{r}\delta)s. \tag{3}$$

Given the peculiarities of my data set—its unbalanced nature (many missing observations towards the beginning and end of the period), the availability of capital and value-added only for Census years, the desire to preserve comparability with the earlier study, and the difficulty of doing elaborate programming inside the Census Bureau, I focus primarily on two major dimensions of the data: levels (in 1967, 1972, and 1977) and growth rates, and eschew any attempt at a complete annual data analysis. The annual data are summarized by computing average growth rates for two subperiods 1957–65 (corresponding to the earlier study period) and 1966–77, based on regressions of the logarithms of the relevant variables on time trends (solving thereby the missing years problem within each of these subperiods).

In implementing such a framework of analysis one has to deal with several serious data problems: missing data, erroneous data and possible erroneous matches, and mergers. Except for *R&D* data, no special effort was made to replace missing values by various imputation procedures. It was my notion that the basic data set represents what the Census did collect, what we actually know, and that any imputation procedure should be done only in the context of a particular research project where its implications for the final analysis could be interpreted. As far as the *R&D* data are concerned, the Census used the shuttle nature of the original questionnaires to fill in many of the original blanks. To the extent that there remain missing values which are not due to the fact that the whole company is missing before or after some date, they were interpolated on the basis of the estimated growth rates (which require at least five good data points within each subperiod). For other variables, missing values were not imputed. It was not possible, within the constraints of this project, to develop optimal imputation procedures. This would have required several repeated passes at the original numbers. Instead, the analysis is based either on reduced "clean" samples or on "pairwise present" correlation coefficient matrices.

From an econometric point of view, we have to deal with the problem of firm effects (or firm-specific left-out variables) and the possibility that the relationships being estimated may not stay constant either across firms or across time. The first is handled by analyzing first differences or growth rates, transformations that eliminate any unchanging effects from the data. The second problem, the problem of differences across firms, is handled in part by calculating a measure of "partial" productivity growth $[BPT = y - (1-\hat{\beta})l]$, using individual firm data on the share of labor in total costs. One can also estimate separate and different parameters for the various industry groupings and include some of the other variables available in the record which might distinguish one firm's environment and response pattern from another's (such as its specialization ratio, size, or vertical integration). The main hypothesis under investigation, that the returns to *R&D* investments may have declined over time, is tested both by comparing estimates based on the more recent data with the earlier results, and by allowing and testing for systematic changes in the estimated relationships between the three available cross sections.

Let us look now at the first set of substantive results. Table 2 reports the results of estimating cross-sectional production functions (equation (1)) separately for each Census year, adding to the standard capital and labor variables a measure of total *R&D* capital accumulated by the firm and two *R&D* mix variables: the fraction of total *R&D* that was spent on basic research and the fraction of accumulated *R&D* that had been financed privately. All the reported estimates allow for 18 to 20 (depending on the subsample) separate industry intercepts. Columns 1 and 3 report estimates that are based on the same number of firms and use the same dependent variables, differing only by the year of observation. Column 2 presents additional estimates for 1972 based on different sample and dependent variable definitions with the main intent being to show that the major conclusions are insensitive to such differences. There are three major points to be made about these estimates. The first is that the stock of *R&D* capital contributes significantly to the explanation of cross-sectional differences in productivity and there is little evidence of a decline in its coefficient over time.[2] There is a minor rise in the estimated coefficient from 1967 to 1972 and a somewhat larger but not really significant decline from 1972 to 1977. Given this particular measure of *R&D* capital, based on a

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[2] Here and subsequently, all statements about statistical "significance" should not be taken literally. Besides the usual issue of data mining clouding their interpretation, the "samples" analyzed come close to covering completely the relevant population. Tests of significance are used here as a metric for discussing the relative fit of different versions of the model. In each case, the actual magnitude of the estimated coefficients is of more interest than their precise "statistical significance."

TABLE 2—NSF-CENSUS STUDY: CROSS-SECTIONAL PRODUCTION FUNCTIONS,
LOG VALUE-ADDED DEPENDENT VARIABLE[a] U.S. FIRMS: 1967; 1972; 1977

	(1)		(2)		(3)[b]	
Variables	1967	1972	1972	1972	1972	1977
ln Employment	.604 (.045)	.622 (.046)	.623 (.035)	.586 (.038)	.578 (.038)	.611 (.039)
ln Capital Services	.224 (.041)	.199 (.044)	.161 (.032)	.234 (.036)	.254 (.036)	.291 (.035)
ln *R&D* Stock (*db*)	.113 (.023)	.135 (.026)	.165 (.019)	.126 (.019)	.115 (.018)	.089 (.017)
Basic Research (*BR*)	.396 (.240)	.340 (.261)	.274 (.215)	.499 (.191)	.517 (.189)	.401 (.189)
Company-Financed Research (*FP*)	.190 (.097)	.247 (.106)	.068 (.100)	.133 (.088)	.138 (.088)	.044 (.084)
N	386	386	652	491	491	491
SEE	.312	.336	.390	.312	.309	.290

Notes: ln Employment = log (total employment-employment of scientists and engineers); ln Capital Services = log of (depreciation plus interest on net assets plus machinery and equipment rentals); ln *R&D* Stock (*db*) = log of the "stock" of total *R&D* expenditures based on a 15 percent per year declining balance depreciation assumption; *BR* = basic research as a fraction of total *R&D*; 1972 in the 1977 equation, 1967 in 1967 and 1972. *FP* = fraction of *R&D* stock "private," company-financed *R&D* stock as a ratio to the total *R&D* stock, as of *t*. All equations include also a constant term and industry dummies. The number of industry dummies used depends on the data set and varies between 18 and 20. Standard errors are shown in parentheses.

[a] Value-added and materials used in research in 1967 and 1972.

[b] Value-added only.

15 percent per year declining balance depreciation formula (the results are insensitive to the particular formula used), the implied average (at the geometric mean of the sample) gross rate of return to *R&D* investment rises in a similar fashion from .51 in 1967 to .62 in 1972 (in col. 1) and falls from .39 in 1972 to .33 in 1977 (in col. 3). In either case the estimated rate of return is quite high and there does not appear to be any dramatic fall in it over time.

The second major finding is the significance and rather large size of the basic research coefficient. It seems to be the case that firms that spend a larger fraction of their *R&D* on basic research are more productive, have a higher level of output relative to their other measured inputs, including *R&D* capital, and that this effect has been relatively constant over time. If anything, it has risen rather than fallen over time. Using the formulation of equation (2) implies a very high premium on basic versus the rest, a $\hat{\delta}$ of between 2.5 to 4.5, a several hundred percent premium on basic research. Before I explore the implications of this result, I want to examine other dimensions of these data and see whether similar effects can be observed there too.

The last major result of interest is the significant positive coefficient on the privately vs. federally financed *R&D* mix variable. This variable is of most import for the older more established firms in subsample 4 (Table 1) but its sign is consistent throughout, indicating a positive premium on privately financed *R&D*, or equivalently a discount as far as federally financed expenditures are concerned. Here the implied premium is smaller, between 50 and 180 percent, but still quite large.

All the above results were based on cross-sectional level regressions that are subject to a variety of biases, the main one being the possibility that "rich" successful firms are both more productive and can afford to spend more of their own money on such luxuries as *R&D* and especially the basic variety. One

Table 3—Growth Rate of Partial Productivity, 1966–77

Variables	$N = 911$			$N = 652$ (*with industry dummies*)	
Constant	.019	.009	.012	–	–
BTRD 6677	.107		.117	.119	
	(.014)		(.017)	(.016)	
BCRD 6677		.095			.106
		(.014)			(.015)
BR 72	.056	.056	.059	.035	.034
	(.017)	(.017)	(.019)	(.018)	(.018)
FP 72	.011	.019	.017	.022	.030
	(.005)	(.005)	(.006)	(.007)	(.007)
SEE	.0383	.0384	.0337	.0305	.0307

Notes: Dependent variable: *BPT* 6677 = trend growth rate of deflated sales minus the trend growth of total employment multiplied by the share of payroll in total sales. *BTRD* = trend growth of deflated total *R&D* expenditures; *BCRD* = same for company-financed *R&D* expenditures; *BR* = basic research expenditures as of fraction of total research expenditures; *FP* = ratio of company-financed *R&D* stock to total; *SEE* = residual standard error. All equations contain also a term reflecting the variance of *R&D* and terms representing the growth of physical capital: age composition and depreciation as of 1972.

can reduce somewhat the possibility of this type of bias by focusing on firm-growth rates, the changes that occurred, rather than on their levels. To the extent that firms have idiosyncratic productivity coefficients that may be also correlated with their accumulated *R&D* levels, considering growth rates is equivalent to doing a "within" firms analysis, one that eliminates such fixed effects from the analysis. The next two tables present, therefore, the results of analyzing the growth in the partial productivity of these same firms during the whole 1966–77 period.

Table 3 presents the results of estimating partial productivity equations in the largest possible sample for which 1966–77 growth rates were computable ($N = 911$) and in the subsample with a successful 1972 Census match. Here again we find my three main results confirmed: the *R&D* growth term and the two mix variables, the basic research ratio, and the fraction of research financed privately all contribute significantly to the explanation of productivity growth.

On the assumption that the growth rate in the stock of *R&D* is roughly proportional to the growth in deflated *R&D* itself, the coefficient of *BTRD* should be estimating the same number as the coefficient of the *R&D* stock variable in Table 2. The results are in fact surprisingly close: about .12 in Table 3 as against .09 to .17 in Table 2. Moreover, there seems to have been no decline in this coefficient relative to the earlier 1957–65 period. In my previous study (1980a), I estimated the same coefficient to be .073. In the current replication and extension of this sample a similar equation for 1957–65 yields a *BTRD* coefficient of .086. Thus, if anything, the coefficient of *R&D* went up between the early 1960's and the early 1970's.

The second major finding of interest is the positive and significant basic research coefficient. It is hard to interpret its magnitude since the approximation outlined in equation (3) breaks down when the average growth rate of deflated *R&D* and of basic *R&D* is close to zero or negative. Consider, however, the following illustrative calculation. Raising the *BR* ratio by one standard deviation, from .026 to .097 at the mean, would increase the rate of growth of partial productivity by close to half a percent per year ($.071 \times .059 = .0042$). This same increase would raise the growth of total *R&D* by .107 for one year and would contribute a

TABLE 4—GROWTH RATE OF PARTIAL PRODUCTIVITY, BY INDUSTRY, 1966–77
(MATRIX 6, TOTAL $N = 991$)

Coefficients of	Coefficients by the Estimated t-Ratio			
	< -1.5	$-1.5-0$	$0-1.5$	$1.5+$
BTRD		2	7	10: Miscellaneous, Industrial Chemicals, Drugs, Stone & Glass, Machinery, Electronics, Electrical Equipment, Transportation Equipment, Scientific Instruments, Non-Manufacturing
*BR*72		5	8	6: Wood & Paper, Other Chemicals, Oil, Machinery, Aircraft, Non-Manufacturing
*FP*72	2	6	7	4: Oil, Rubber, Electronics, Aircraft

Notes: All equations contain also a term reflecting the variance of *R&D* and terms representing the growth of physical capital: age composition and depreciation as of 1972.

once-and-for-all increase in the level of productivity of .0125. Discounting the more "permanent" effect of basic research by a real interest rate of .05 yields an "equivalent" one-year effect of .084, or a 7 to 1 ratio in favor of basic research! If one allows for industry dummies which in this formulation represent separate industry trend rates of disembodied technical change, the effect of basic research is cut by about 50 percent, implying perhaps that a signficiant fraction of the estimated effect comes from spillovers that diffuse throughout the industry. Note that it is the only coefficient that is affected substantively when separate industry dummies are allowed for. Nevertheless, even a 3.2 to 1 ratio is quite high!

The third finding is the significant positive premium on company-financed *R&D*. Here too the implied premia are quite high, but given that the mix variable is defined in terms of stocks rather than flows, the calculations are more cumbersome. Consider starting from a zero growth position and a .7 ratio of private to total *R&D* stock. To move this fraction from .7 to .75, one would need to raise the private stock by 29 percent and the overall stock by 20 percent (without reducing absolutely the stock of federally financed *R&D* capital). There are different possible investment paths that would achieve this goal and would have somewhat different present value consequences. If one roughly doubled the rate of privately financed *R&D* expenditures, from the previous replacement level of .105 (.7×.15) to .205, one could achieve this target in slightly over two years. Ignoring discounting, this would lead to a once-and-for-all growth in productivity of .024, due to the growth in the total stock of *R&D and* a .0011 permanent increase in the rate of growth due to the shift of the fraction private ratio from .7 to .75. The present value of this second term is about .022, or of the same order of magnitude as the first term. That is, raising the stock of *R&D* by 20 percent but shifting it all into the private component doubles the effect of such dollars.

There are problems, however, with such an interpretation. If private *R&D* expenditures contribute more to productivity growth, one might have thought that when they are substituted for the total *R&D* growth measure, they might fit better and also have a higher coefficient. But that is not the case as can be seen from the results presented in columns 2 and 5 of Table 4. The total *R&D* measure does a little bit better both in terms of fit and in the overall size of its coefficient, implying that the contribution of federal dollars is not zero. That is perhaps what one should expect. Most of the direct output of federal research dollars is "sold" back to the government at "cost plus" and is unlikely to show up as an increase in the firm's own productivity. Thus all that one could expect to measure here are the within-firm spillover effects of such expenditures. What we may be detecting is that such effects are indeed present and positive, but we should not have expected them to be of the same order of magnitude as would be the case for the firm's own investments in improving its productivity or profitability.

There are a number of econometric questions that can be raised about the robustness and sensitivity of such results. I will discuss only a few of these here. The most obvious question arises from the fact that even though I allowed, in the growth rates version, for separate firm intercepts and different industry trends, I am still assuming common *R&D* and the conventional capital coefficients across rather different industries. This is done from necessity rather than as a virtue. Estimating the same models industry by industry reduces the sample sizes drastically and raises greatly the relative noise level, making it rather hard to interpret the resulting estimates. Nevertheless, these estimates, which are summarized in Table 4, are quite consistent with the earlier story: 17 out of the separately estimated 19 coefficients for the *R&D* growth variable are positive and more than half of them are statistically significant at conventional significance levels. Similarly, the coefficients of the basic research ratio variable are positive in 14 of my 19 industries and signficant in over a third of them. The fraction private variable is less robust to the division of the sample into industries, with more than half of the coefficients still positive, but only 4 of them are statistically significant within particular industries. Two of these industries are indeed the ones where one would expect to find such an effect, aircraft and electronics, industries where the bulk of federal monies is spent. Nevertheless, it seems that the effect that is being caught by the fraction private variable has an important industry component, something that had been already noted in my earlier study (1980a), as does also the effect associated with the basic research variable, though to a lesser extent.

A number of other versions were computed using the growth in capital services rather than the depreciation and age composition variables that had been used to keep the results comparable to the earlier study, and the growth in *R&D* "capital" rather than the flow (and also different definitions of such capital). I also estimated versions using the "intensity" form for the *R&D* variable, to make it more comparable to other studies in the literature (my paper with Lichtenberg; Mansfield; and others).[3] By and large the results of these alternatives were somewhat weaker but not substantively different. Perhaps the most interesting alternative estimate is the intensity version using the growth of capital between 1967 and 1972 as its capital measure:

$$(4)\quad \begin{array}{llll} BPT6677 = \ldots & \underset{(.069)}{.243ACRS} + \underset{(.024)}{.045ABR} & & \\ & + \underset{(.130)}{.180DLCS} & & \underset{(\text{Subsample } 4)}{SEE = .0316} \end{array}$$

where *ACRS* is the average company *R&D* to sales ratio, averaged over 1967 and 1972, *ABR* is a similar average basic to total *R&D* ratio, and *DLCS* is the rate of growth in deflated capital services between 1967 and 1972. This version is closest in form to the equation estimated by Mansfield on much smaller samples. The basic results are similar, however. Basic *R&D* is a signficant contributor to productivity growth with an implied basic to company premium of about 5 to 1 (given an average *R&D* to sales ratio of .035).

The final set of results to be presented here, in Table 5, relate to the relative profitability of our firms in 1972 and 1977. The dependent variable, *GRR*, is the ratio of gross profits (value-added minus labor costs and plus *R&D*) to total gross fixed assets. The independent variables include the ratio of *R&D* capital (undepreciated) to total fixed assets and our ubiquitous *R&D* mix variables: the basic research and fraction private

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[3] The intensity version uses the fact that $\alpha = (\partial Q/\partial K)(K/Q)$ and reexpresses $\alpha\dot{K}/K$ as $\rho[R/Q]$, where $\rho = \partial Q/\partial K$ is the marginal product (gross rate of return) of *R&D* capital and it has been assumed that $\dot{K} = R - \delta K \approx R$, i.e., either $\delta = 0$ (no depreciation) and/or initial K very small. This formulation has the advantage that it does not impose the assumption of a constant *elasticity* across different firms, replacing it instead by the, possibly more plausible, assumption of the constancy of rate of return.

TABLE 5—GROSS PROFIT RATE REGRESSIONS
(*GRR* = (Value-Added-Payrolls + *R&D*)/Gross Assets))

		Coefficients of			
Dependent Variable and Sample Size	Constant	*R&D* Capital to Total Fixed Assets Ratio	Basic *R&D* Ratio	Fraction Private	*SEE*
GRR 72					
N = 652 (a)	.144	.088	.344	.107	.262
	(.049)	(.012)	(.144)	(.048)	
(b)		.060	.187	−.012	.237
		(.013)	(.138)	(.052)	
N = 491 (a)	.117	.080	.514	.154	.264
	(.052)	(.013)	(.139)	(.051)	
(b)		.061	.366	.074	.227
		(.015)	(.138)	(.057)	
GRR 77					
N = 491 (a)	.341	.031	.402	.033	.313
	(.064)	(.019)	(.187)	(.068)	
(b)		.004	.261	−.028	.292
		(.022)	(.187)	(.077)	

Notes: (a) Regressions do not contain industry dummies; (b) do.

ratios. Even though the dependent variable is quite different, the overall results are rather similar to the earlier ones. The *R&D* capital variable is positive and almost always statistically significant through its coefficient is a bit low if it is to be interpreted as a rate of return to it. The basic research variable is both large and significant though possibly too large to be credible. Given that the ratio of total *R&D* capital to total fixed assets is only about .05 on average, the 1972 coefficients imply a δ of about 30 to 60. The fraction private ratio also contributes positively to profitability but its effect largely disappears once industry differences are allowed for. The results for 1977 are weaker than those for 1972, the residual variance is signficantly higher, but they too suggest the importance of basic research even in this context.

A similar analysis was performed using an estimate of the net rate of return as the dependent variable, subtracting depreciation from the numerator of *GRR* and using a net stock concept for the denominator and also in the definition of the *R&D* capital variable. While the fit was significantly worse when using this definition of the dependent variable, the overall results were rather similar. The net return version was also available for 1967 and the results using it indicate a relatively constant and significant coefficient for the basic research ratio while the coefficient of the total *R&D* stock rises from 1967 to 1972 and than falls again in 1977 (from .11 to .16 and down to .06). It is doubtful whether these fluctuations represent real trends or, more likely, reflect the larger noise level in the 1977 data and the changing composition of these samples. In any case, the profitability regressions are consistent with the productivity level and productivity growth rate based results described earlier (Tables 2, 3, and 4).

III. Discussion and Summary

There are three major findings in this paper: *R&D* contributed positively to productivity growth and seems to have earned a relatively high rate of return; basic research appears to be more important as a productivity determinant than other types of *R&D*; and privately financed *R&D* expenditures are more effective, at the firm level, than federally financed ones. These findings are not entirely new. The first finding has been documented in a number of earlier studies (see my 1980a, b papers; my article with Mairesse, 1984; A. N. Link, 1981a; and others). What is new in this paper in this regard is a confirmation of this finding on a much larger and more recent data set. It also presents evidence for the view that this effect has not declined significantly in recent years, in spite of the overall slowdown in productivity growth and the general worry about a possible exhaustion of technological opportunities.[4]

The evidence for a "premium" on basic research is much more scarce. The major previous paper suggesting this type of a result is Mansfield which uses aggregate data for 20 industries for 1948–66 and data for 16 firms during 1960–76, and finds a signficant premium on basic research, on the order of 2 to 1 at the industry level and 16 to 1 at the firm level. (See also Link, 1981b, for similar results for 1973–78 based on data for 55 firms.) In this paper I get similar though somewhat smaller effects at the firm level, using a much larger and more representative sample. I also find that differences in levels of productivity and profitability are related to differences in the basic research intensity of firms.

Such findings are always subject to a variety of econometric and substantive reservations. In this context the two major related issues are simultaneity and the question of how major divergences in private rates of return persist for such long periods. It is possible to argue that it is not *R&D*, or its basic research component, that causes firm "success" as measured by productivity and profitability, but rather that success allows firms to indulge in these types of luxury pursuits. It is difficult to argue about causality on the basis of what are essentially correlational data. It is possible to use simultaneous equation techniques to estimate such models, but then the argument shifts to the validity of the exogeneity assumption for the particular instruments. In the context of my specific data set, it is hard to think of any valid instruments except for possibly lagged values of the same variables, which raises some problems of its own. The best evidence for the notion that these results are not entirely spurious is provided by the growth rates where the individual firm levels are partialed out of the analysis. But, here too, one could argue about the impact of common unanticipated "luck" elements. Unfortunately, it is unlikely that one could use lagged growth rates as instruments, since there is very little correlation in growth rates over time at the firm level. While an attempt will be made in further work with these data to estimate more extended simultaneous equations versions of such models, I am not too optimistic as to what can be accomplished in this regard. The evidence presented here should not be interpreted as "proving" that *R&D*, and especially its basic component, are important for productivity growth but rather as presenting some prima facie evidence in support of such an interpretation. In this sense it is an exercise in economic rhetoric (Donald McCloskey, 1983).

It is even more difficult to respond to the theoretical a priori argument that such results cannot be true since they imply widely differing rates of return to different activities under the control of the same firm. One's response to this depends on one's views as to the prevalence of equilibria in the economy. While it is likely that major divergences in rates of return are eliminated or reduced in the long run, the relevant runs can be quite long. *R&D* as a major component of firm activity was undergoing a diffusion process in the 1950's and 1960's and may not have reached full equilibrium even by the end of our period. This may be especially true of the basic research component where the risks are much greater and the uncertainty introduced by changing government policies and the changing economic environment make it quite difficult to decide what is the right level for it.

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[4] The finding that the coefficients in a logarithmic regression have not declined over time does not dispose of the possibility that there could have been an overall loss in accumulated knowledge capital due to accelerated obsolescence. A proportional decline in the effectiveness of past capital or in the rate that *R&D* is converted into new knowledge capital need not show up as decline in the slope coefficient, it would get absorbed into the shifting constant. Disproportionate shifts should, however, have an impact on the estimated slope coefficient. Also, a pure obsolescence shock to old knowledge capital would have called forth an increase in the rate of *R&D* expenditures, something which has not been observed in the data. I am indebted to M. N. Baily for this point.

A somewhat different version of this argument would claim that the world is indeed in approximate equilibrium but that different firms face different opportunities for doing research, basic or otherwise, are in different ecological niches, and hence have different coefficients in their "production functions." This would explain why different firms are observed to spend different amounts on *R&D* while actually earning about the same rate of return on it. When a constant coefficients production function is fit to such data, it will fit because it is approximating a market equilibrium relation. If the level of *R&D* invested were independent of the coefficient, then such a function would just reproduce its average share and not produce any evidence of excess returns. But if, as is reasonable, *R&D* is invested optimally with firms which have better opportunities, higher coefficients, investing more, this will induce a positive correlation between *R&D* and its individual coefficient and lead to an upward bias in the estimated "average" coefficient.[5] The resulting "larger" coefficient, larger than the observed factor share, will be interpreted, wrongly, as implying a higher rate of return than is actually prevailing at the individual level.

This argument may be recognized as a version of the earlier attacks on the Cobb-Douglas production function combined with a random coefficients interpretation of the same phenomenon. In its extreme form it is testable. Since there are time-series data available for individual firms, one could try to estimate individual firm parameters and check whether they are in fact distributed as is predicted by this particular argument. While individual parameters are unlikely to be well estimated, given the relative shortness of the available time-series, the parameters of the distribution of such coefficients might be estimable with more precision. I intend to pursue this possibility in future work.

To restate again the major points of the paper: a newly available body of data on all the major firms performing *R&D* in the United States has been examined and evidence has been presented for the proposition that *R&D* contributes significantly to productivity growth, that the basic research component of it does so even more strongly, and that privately financed *R&D* expenditures have a significantly larger effect on private productivity and profitability than federally financed *R&D*. These findings are open to a number of reservations. Nevertheless, they do raise the issue that the overall slowdown in the growth of *R&D* and the absolute decline in basic research in industry which occurred in the 1970's may turn out to have been very costly to the economy in terms of foregone growth opportunities.

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[5] A positive correlation is not enough, by itself, for a positive bias. The weight of an individual firm slope coefficient in the cross-sectional estimate is proportional to the *square* of the deviation of *R&D* stock from its mean. A positive correlation between levels does not translate itself directly into a positive correlation between the level of one variable and the square of the other, except for certain skewed distributions. Since we do not observe the individual coefficients directly, it is rather difficult to check out this conjecture.

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DISCUSSION

Mr. **Fuqua**. Thank you very much for a very interesting paper this morning.

You note that economics, while it doesn't have all the answers, that it can provide and be a useful aid to priority formation and decisionmaking. I guess my question is, can economics help us decide what the total Federal expenditure for research should be, and if so, how could it do that?

Dr. **Griliches**. I don't think you can have a magic answer in terms of a percentage number in an overall sense. I'm not even sure that that's the right way of asking the question, because the Federal research budget is made up of a variety of different activities and under the research label many different things are involved. Currently over half of the research budget, Federal research budget, is in defense and space. I don't think that one can easily, a priori, decide how much money we should spend on research in defense. I think it is a political and economic judgment call.

I think it doesn't really help to lump that kind of research with, for example, the maintenance of the scientific and educational base of the country on which the possibility of doing that kind of research will actually depend on many generations to come. So I think it makes more sense, to my mind, to discuss components of this budget separately and face up to priorities in the question of size of the overall Federal budget in the long run.

I am conscious that, in a sense, I am weaseling out of answering your question directly, but I think in a way I want to state that economics does not have easy answers. It pushes you back to saying "Well, what do you want to accomplish?" I can tell you what it might cost; I can tell you what might be the consequences of making the move in this direction or in another. But a choice will have to be made. There is no easy decision between defense today and a question about, let's say, the support of our elderly population today. You know, we are making some of these substitutions now. The examples may be far fetched, but I'm saying the whole Federal budget, the taxpayers money, is being allocated to different uses. There is ultimately an opportunity cost for every dollar, and people who are making a choice have to be cognizant of that if they're to make it rationally.

What is happening is basically we're having special groups making special cases for special projects, hoping that their voice is heard the loudest and survive the general onslaught.

Mr. **Fuqua**. The OTA study noted that some studies have shown there is a strong coordination between the manufacturing industry between R&D expenditures and productivity increases. It is not clear which comes first. I think you have been one of those whose analysis has cited that it could be the other way around, that because a company made higher profits and productivity had increased, they had put more money into R&D. How do you determine the cause and effect of this?

Dr. **Griliches**. Well, I'm slightly embarrassed by that quotation that OTA picked up. I was making it in a sense as recognizing that somebody would come and criticize some of my work with that possibility. I did not entertain that possibility as a very high probability, but I was basically covering my flank in advance by saying—you know, somebody may say all right, this is correlational data, and in correlational data there is always a question of whether "post hoc" is "proper hoc"—you know, whether it's because it comes after it follows that it has been caused by the earlier event. I think in much of social science, where you do not have direct experiments, you have inferences which are based on a chain of arguments which have weak links in them.

I would say that I think the timing evidence, by and large, indicates reasonably clearly that most of the causal direction flows from R&D to productivity, rather than the other way around. Now, there is no doubt in my mind that in good times, the expectation of good times, has an important impact on the availability of resources to R&D. But the fact there is a feedback does not, by itself, negate the contribution of R&D to productivity growth.

I think the several efforts that I and others have done, to try to analyze these things with lags, with using more complicated simultaneous equations techniques, by and large have not overturned the main findings of a relatively high rate of return, social rate of return, to R&D expenditures in the manufacturing sector.

Mr. **Fuqua**. As the cost of doing science, and especially big science, continues to increase, how can we tell when we're approaching the law of diminishing returns?

Dr. **Griliches**. We probably are—you know, we will not escape the law of diminishing returns, but that doesn't mean that you stop investing. Just as the fifth pound of fertilizer on a lot of land may not produce the same increment as the first one, that doesn't necessarily mean you shouldn't apply it. There is a calculus here. I think that is a very real issue—there are two levels.

First of all, even with diminishing returns, we get diminishing returns in various directions, and even with diminishing returns it will pay to invest in research as long as it has positive returns, and these returns are higher than in other areas.

The other point is that, by and large, by investing in knowledge, we have been able to expand the horizons and the opportunities to make further investments. That is one of the things that has kept the rate of return to investment, in general, up, because if you think about the rather large capital accumulation in this country over time, you could easily anticipate that we will have diminishing returns all along. It is a hope, but it is not a certainty, that investment in knowledge will, in fact, prevent the rate of return to investment from falling in the long run.

One of the current debates for which we do not have good statistics is, in fact, whether or not the past decade of slow productivity growth is a reflection of diminishing returns or a reflection of bad macroeconomic policies. I think the jury on this one is still out.

Mr. **Fuqua**. I wish we had time to go on. We have some other distinguished people waiting, but we thank you very much for being here and sharing with us your very interesting paper.

Dr. **Griliches**. Thank you.

Mr. **Fuqua**. We hope to have you back again, Dr. Griliches.

[Questions and answers for the record:]

SCIENCE POLICY TASK FORCE

Hearings on Research Funding as an Investment

April 30, 1986

Followup Question for Dr. Griliches

In your testimony, you discussed the law of diminishing returns in terms of adding "the fifth pound of fertilizer." Since there is opportunity cost or value for every dollar we invest, how can we determine whether our wisest investment is in that fifth pound of fertilizer, or some entirely different investment with its potential return? Bear in mind that these alternatives are both in basic research and so uncertainty shrouds the outcome of both choices.

In my original response I assumed that the decision facing you was a *marginal* one: to approve this or that project, or to fund some particular program and I was reminding you that there is an opportunity cost of funds. One way of thinking about it is to develop the notion of the social rate of return on the best currently *unfunded* projects or expenditures. Any particular new project should be at least as good as that. In that answer I meant to say that if the rate of return on the "fifth pound of fertilizer" had declined below the current social opportunity costs, the money would be better spent on other projects, those exhibiting potentially higher rates of return.

Of course, the actual implementation of such an approach is not at all easy, since there is great uncertainty about the actual outcome of a particular project and hence also about its potential rate of return. Some of it could be overcome if the uncertainties could be quantified and some "risk-adjusted" rate of return computed. There is a whole literature of "cost-benefit" analyses which deals with this range of issues. Not much of it can be made operational for the sorts of decisions that you face (such as optimal basic R&D budgets vs. defense expenditures). Nevertheless, the *ideas* behind such a calculation should inform your decision making: That in some areas returns are likely to be greater than in others and that one can make some rough guesses of the orders of magnitude involved, that costs should be carefully counted, that benefits should not be double counted, and that every dollar has an opportunity cost. Using the framework of such formulae and even guessing about its various components still provides a discipline to and a mode of discourse for such decisions.

PANEL: DR. ALI SEIREG, EBOUGH PROFESSOR OF MECHANICAL ENGINEERING, UNIVERSITY OF FLORIDA, GAINESVILLE, FL; AND DR. WASSILY LEONTIEFF, UNIVERSITY PROFESSOR, NEW YORK UNIVERSITY, AND SENIOR SCHOLAR, INSTITUTE FOR ECONOMIC ANALYSIS, NEW YORK, NY

Mr. **Fuqua**. Our next two witnesses will be Dr. Leontief and Dr. Seireg. You may proceed, Dr. Seireg.

[A biographical sketch of Dr. Seireg follows:]

Dr. Ali Seireg

Received B.Sc. in mechanical engineering from the University of Cairo in 1948 and Ph.D. from the University of Wisconsin in 1954. Lectured at Cairo University from 1954 to 1956. He then joined the Falk Corporation where he served as a research engineer, then as advisory engineer in 1956, and as a consultant since 1960. He became a professor of theoretical and applied mechanics at Marquette University from 1960 to 1965 and went to the University of Wisconsin as a professor of mechanical engineering in 1965. He is also a biomechanics consultant for the Veterans Administration Research Center since 1964 and served as a consultant to the Industrial Bank of Kuwait, the U.S. State Department, and the National Science Foundation.

He authored a book on mechancial systems analysis, edited and contributed chapters to several books and published over one hundred and eighty papers on various facets of automation, dynamic systems, friction lubrication and wear, design optimization and biomedical engineering. He has supervised more than 100 M.S. and Ph.D. dissertations. He has given numerous talks and seminars throughout the country and abroad. He served as advisor to the World Conference on Industrial Tribology, chaired the Design Automation Committee of ASME, the first ASME International Conference on Design Automation, chairman of the ASME Century II International Computer Technology Conference, vice chairman of the technical coordinating committee of the ASME Century II Emerging Technology Conferences, co-chairman of the Fifth World Congress of Theory of Machines and Mechanisms, and is chairman of the United States Council of the International Federation of Theory of Machines and Mechanisms. He also served as a chairman of the executive committee and papers review, and Senate for the ASME Design Engineering Division, and a member of the Research Committee on Lubrication, the Automation Research Council, ASME Policy Board Communications, chairman of the Computer Technology Committee and founding chairman of the ASME Computer Engineering Division. He was elected vice president, Systems Design in 1981 and re-elected to serve in this capacity until July 1985. He also organized and serves as editor of the new ASME Hybrid Journal on Computers in Mechanical Engineering, CIME. President, Gear Research Institute, 1983-, ASME senior vice president and chairman of the Council on Engineering 1985–87.

Dr. Seireg is the recipient of the 1970 George Westinghouse Award of the American Society for Engineering Education, the 1973 Richards Memorial Award of the American society of Mechanical Engineers, the 1974 E.P. Connell Award of the American Gear Manufacturing Association, the 1978 Machine Design Award of ASME, the Centennial Medal of the Design Engineering Division of ASME and four other Centennial Awards for Service to ASME. He was also awarded the 1982 Benjamin Reynolds Award of the University of Wisconsin.

He is registered professional engineer in Wisconsin; an academic member of AGMA, member of ASME, ASEE, SESA, Sigma Xi and Pi Tau Sigma.

Biographical listings: Who's Who in Computer Education and Research, Engineer of Distinction, Wisconsin Men of Achievement, Outstanding Educators in America, Who's Who in the Midwest, Who's Who in Engineering, Dictionary of International Biography, Cambridge 1980, Who's Who in America, 1983, Men of Achievement, Who's Who in the World, 1980, Two Thousand Notable Americans 1948–85. Who's Who on the Frontiers of Science and Technology International Book of Honor.

STATEMENT OF DR. ALI SEIREG

Dr. **Seireg**. Thank you very much, Mr. Chairman, for giving us the opportunity to speak in front of your committee. We would like to address the concerns of your committee which are the concern of, I am sure, many, many people in this country also, in a positive and constructive manner.

I speak to your committee as an engineer, a researcher, and also a person who has been active for many years in the American Society of Mechanical Engineers, which integrates most of the activities in the field of mechanical engineering in the country.

We are very, very concerned about the investments in research and their return, because engineering plays a central role in productivity and economic development. Engineering integrates science and economics, to answer human needs. Engineering utilizes and stimulates research and science.

I will be deviating from the written statement, because I believe it's on the record.

Mr. **Fuqua**. Yes, we will make it part of the record.

Dr. **Seireg**. All right, it will be part of the record. But I would like to speak in general, so I will leave time for questions.

The engineers have been concerned since ancient times in the economic aspects of their work. In reading the documents I received from the Office of Technology Assessment, and the concern of your committee on evaluating the science policy of the country, I must say that everything I read was heartwarming because it explained the problem in great detail and put on paper the concerns of many, many people.

Also, this activity did an excellent job in evaluating the existing conditions. Unfortunately, what I see missing in this document—and I'm sure that's what your committee is concerned about—is a search for a solution. What I would like to share with you today is what we think is an effort toward that solution.

We, as engineers, have been concerned for over a century of economic production. It was interesting today in the newspaper, USA Today, that I read an account by Mr. Shingo, who is a Japanese executive, a keynote speaker in the productivity conference yesterday. The article is entitled "Japanese Executive Gives U.S.A. a WakeUp Call." Fortunately, we are awake and we are trying to do something. He also referred to the basis for their work on productivity, and that was the work of Frederick Taylor, who happened to have been the president of ASME at the turn of the century. So we have been concerned and we have responsibilities as engineers to worry about economic factors.

I would like to share with you a new development. We have plans for an Engineering Economics Institute that would provide quantitative answers to questions such as the value of technology, the value of science, and return on investments in a particular area of science and technology. It will also answer questions such as lost income from not applying resources to areas of science and technology.

After a long search we decided the work of Dr. Leontief, the distinguished Nobel laureate in economics for his input-output models, would be the most appropriate to answer our concerns, and I am hopeful the concerns of your committee.

Dr. Leontief's model has provided excellent predictions and excellent information for decisionmakers to use. I will give a few examples. He studied the effect of corrosion on the economy. He found that $82 billion were lost in 1975 from corrosion. Of this, $33 billion were avoidable with existing technology. Forty-nine billion were unavoidable by existing technology but could be avoided if research had been done in this area. So these are numbers that would give guidance to the importance of science and technology.

Another study dealt with the cost of fracture to the economy. The cost of fracture to the economy was $99 billion in 1978. Of that, $29 billion was avoidable with existing technology. The rest could be avoided with more research in the area.

His model has been used to help guide investments in new technology. The Japanese Government utilized 200 people to develop the input for the model so it can help give them guidance to areas of investment for technology.

The time is ripe to do work in economic analysis in a quantitative manner. We have experience with large-scale systems. We have large-scale computing facilities. We have many interested and distinguished engineers and economists, and we have deep concern on the systematic degradation of our worldwide competitiveness. So the time is ripe. We have received significant interest in this endeavor we are embarking on.

I don't want to take too much of your time. My written statement is on record. But I would like to at this point introduce Dr. Leontief, who will speak about his model to your committee.

Dr. Leontief.

[The prepared statement of Dr. Seireg follows:]

Statement of

Ali Seireg

Before the Science Policy Task Force
Committee on Science and Technology

U.S. House of Representatives

Hearings on
"Reseach Funding as an Investment"

April 30, 1986

Mr. Chairman and Members of the Task Force:

My name is Ali Seireg and I am a professor of mechanical engineering at the University of Florida. I am very pleased to have the opportunity to participate in your hearing on "Research Funding as an Investment." My comments will focus on what additional research is needed to increase our capability to measure the return on investment for research and development. I shall base my remarks on my own experience as a mechanical engineer, a researcher and an active member of the American Society of Mechanical Engineers. However, my views are solely my own and do not represent those of ASME or the University of Florida.

Because of our role in the development and use of new technologies, engineers have always been concerned with and sensitive to economic effects of new science and technology discoveries. I suggest that by focusing on the integration of engineering and economics, we can conceptualize the problem of measuring the return on investment for R&D and how to best address the problem. This integration involves three steps: First, engineers need to provide the quantitative methodology by which a product manager will be able to better evaluate technology alternatives and thereby make a better decision whether a new process or new equipment should be adopted. In the dynamic worldwide market place, economic considerations usually dictate the choice.

Second, with the insight generated from this integration of engineering and economics and better understanding of their interactions, we can then help to upgrade and refine the economic models of the nation and the world. Industry has become such an important factor in the overall economy and changing technology has become such a dominant influence on the health of industry that any upgrading and refining of such models cannot be accomplished without the continuous considerations of the engineering and technical implications. In such an effort, economics and engineering must be treated as two intimately interacting parts of a single-integrated system.

The third effort, which relates directly to the focus of this hearing, is to attempt to develop a method to measure the return, in monetary terms, of our investment in research projects. We recognize that the government's return on research should not be counted only in dollars; nevertheless, it would be very useful to eventually have a quantitative guide.

I believe that the most effective approach to developing such a method is to derive it as a byproduct of the work towards the first two steps. In other words, the effort will be more meaningful and more productive after we have thoroughly integrated the two interacting parts of the system: engineering and economics.

However, the task to accomplish these three objectives is not a simple one. The complexity and difficulty are so well known that I should not spend the time here to reiterate. On the other hand, the degradation of U.S. industrial competitiveness requires us to make a substantive attempt to reverse the trend and manage the change.

A considerable amount of good work in economic analysis and modelling has already been done. The need is to clearly understand and define the interfaces and interactions between technology and economics, to develop engineering-economics models with real and useable inputs and outputs, and to take into account a sufficient number of parameters and transfer functions to result in a meaningful and practical tool.

This nation has gained a great deal of practical experience over the recent decades in the handling of large and complex systems such as the various weapons systems, the space systems and the weather forecasting systems. Such experience in systems analysis, systems engineering, systems management and oversight are directly useful in the engineering economics effort.

In addition, the availability of fast, large-scale computers and artificial intelligence techniques which are now capable of tracking the necessarily large number of inputs, outputs, parameters and relations, now make the task more manageable.

As you may know, the Japanese Science and Technology Agency has developed programs for identifying economic and social needs and establishing priorities for various research and development projects that can meet these needs. This activity has proved very successful in providing useful forecasts which enable officials in government, industry and academia to make long-term decisions on research and development priorities.

To summarize, I generally agree with the conclusions of the OTA study, "Research Funding as an Investment: Can We Measure the Returns," that evaluation of the benefits of Federal R&D expenditures is extremely difficult. This is particularly true for government investment in basic research where returns may be years in the future. However, it is my firm belief that the practical development of quantitative methods for evaluating the economic benefits of investing in particular areas of applied research and technology can now be undertaken with the prospect of steady and tangible progress.

In closing, I wish to point out that a Center for Engineering Economics is being launched in a cooperative effort by the American Society of Mechanical Engineers and the Institute of Economic Analysis at New York University. The next witness, Dr. Wassily Leontief, who was awarded a Nobel Prize in Economic Science, will lead the program. I have great hopes that this endeavor will contribute substantially to assisting government and industry in evaluating investments in R&D. I have enclosed as an appendix a description of the ASME Center for Engineering Economics.

Again, Mr. Chairman, I appreciate the opportunity to participate in this hearing and I will be happy to answer any questions.

Appendix

ASME Center for Engineering Economics

1. The pace of technological change coupled with international competition has made deciding among technology options much more important. To remain competitive the United States must strengthen the connection between engineering and business decisions or be satisfied with an ever decreasing share of the benefits of technology based production.

The connection can be characterized as Engineering Economics. Engineering Economics includes the economic considerations and consequences of the practice of all engineering, but more specifically mechanical engineering. Mechanical engineering is global. Regardless of the underlying discipline of a new product or process, almost invariably, the mechanical engineer is called upon for concept innovation, safe and successful design, and production.

To tie together the major trends affecting ASME's strategic purposes, the Council on Engineering proposes to establish the ASME Center for Engineering Economics. The Center is planned as a self-supporting research activity.

2. The ASME effort on Engineering Economics is being made under the leadership of Dr. Wassily Leontief, a Nobel Laureate at the Institute of Economic Analysis at New York University. Dr. Leontief is the

creator of Input-Output Analysis. ASME has surveyed the many different approaches used by economists and has found the approach developed by Dr. Leontief to be most suitable for a systematic and data-based treatment of this crucial subject. Among the many awards received by him is the Order of the Rising Sun, the highest civilian award of Japan that can be awarded to a foreigner, for having "through his theory of Input-Output Analysis and its concrete application — greatly contributed to Japan's economic growth." Other prominent economists and engineers will contribute their efforts as needed.

After the initial basis of the study is defined, ASME will invite other interested science and engineering bodies to join in and contribute in their areas of interest and knowledge. The American Association for Advancement of Science has already expressed interest in supporting ASME in this endeavor.

3. The primary focus for the ASME effort is to develop a computer-based expert system for use in decisions related to the interactions between research, innovation and economics. The optimum use of human, material and capital resources is at the core of this work. With the use of reliable data, past history and expert knowledge, Dr. Leontief's input-output economic model can be expanded to accomodate modern developments in artificial intelligence and inference techniques to provide guidance related to the economic aspects of research and engineering. Questions related to research as an investment could accordingly be addressed in a quantitative manner.

4. All pertinent studies, data and statistical evaluations can be digested, organized and incorporated in the program. This includes the studies undertaken in the U.S.A. (e.g. Office of Technology Assessment, NSF, DOE, and the National Academies, etc.), Japan and Europe.

5. With the incorporation of suitable research and technology data base, attribute data base, and inference logic, a computer based input-output model can be developed to provide answers to many of the questions which so far have been answered subjectively by experts or by peer judgement. We believe that the results of this project will have widespread implications for public as well as corporate policy.

6. The program could be used to investigate the value added to a product, or the economy in general, from an investment in a relevant research and development activity. In addition, questions such as the value lost to the economy by the lack of research in critical areas, ineffective means for the transfer of research results and the depreciation of knowledge capital could be answered quantitatively.

[A biographical sketch of Dr. Wassily Leontief follows:]

Dr. Wassily Leontief

Dr. Wassily Leontief studied at the University of Leningrad, from which he received the degree of learned economist in 1925. He continued his studies at the University of Berlin, which granted him the Ph.D. in economics in 1928.

Dr. Leontief first came to the United States in 1931 at the invitation of the National Bureau of Economic Research. He joined Harvard University, created the Harvard Economic Research Project in 1946, and served as its director until 1972. In 1953 he was appointed Henry Lee professor of economics at Harvard, where he remained until 1975 when he joined New York University as professor of economics. He founded the Institute for Economic Analysis (IEA) at NYU in 1978 and served as its first director until 1985. He currently holds the rank of university professor at NYU and is IEA's senior scholar. Dr. Leontief is an Officer of the French Legion d'Honneur (1968), a recipient of the Order of the Rising Sun (Japan, 1984) and of the French Order of Arts and Letters—Commandeur (1985). He was awarded the West German Bernard-Harms Prize in Economics (1970), and has received many other honorary awards. In 1973, The Royal Swedish Academy of Sciences awarded Dr. Leontief the Nobel Memorial Prize in Economic Science "for the development of the input-output method and for its application to important economic problems."

Dr. Leontief was commissioned by the United Nations to explore the developmental prospects of the world economy up to the year 2000, determining the possibility of narrowing the gap between developed and less developed countries. His research team has constructed a large, multi-regional input-output model of the world economy, and several even more detailed models of the U.S. economy. He is currently directing, under contract with the Italian Government the preparation of a comprehensive "general transportation plan" for Italy.

Dr. Leontief is the author of eight books including the Structure of the American Economy, 1919-1929 (1941, 1953), Input-Output Economics (1966, 1986), Essays in Economics (1966, 1985), The Future of the World Economy (1977), and the Future Impact of Automation on Workers (with F. Duchin, 1986) as well as over 200 scholarly articles.

STATEMENT OF DR. WASSILY LEONTIEF, UNIVERSITY PROFESSOR, NEW YORK UNIVERSITY; AND SENIOR SCHOLAR, INSTITUTE FOR ECONOMIC ANALYSIS, NEW YORK, NY

Dr. Leontief. Mr. Chairman, I am pleased to have an opportunity to tell you about what I think is a promising approach. I will limit myself not to all questions which are raised by you. Having published my first scientific article in 1925, which is about 61 years ago, and continuous research in the same subject going on, I certainly have an opinion about it. But what one wants to have is not opinions. One wants to go to facts. I will try to present to you at least the principle of this approach.

Obviously, I cannot describe in detail, but I think it is rather simple to explain what the essence of the problem is and what research one can use to find answers to this question.

Technology is the background of an economy. But it is not one technology. Each industry, each activity, has its own technology, but it is a combination of technologies that determines the effect of technological change.

If we did not have progress in the plastics industry, we would not construct new types of automobiles. In other words, one technology helps other technologies. You can understand the significance of any particular improvement in any branch of our economy only by taking into account how it ties up with others.

In other words, the economy is a system. It was always said that you can either study with trees or with forests. The modern approach we can describe as the forest of important trees, and study the relationship, which is really what is the modern systems approach for use of computers and modern information systems.

Let me show how I would describe the picture of the economy. You cannot read the details of a chart, but I can explain to you the meaning of it. Over on the left-hand side we list all the industries—in this case, it's a rather rough industrial specification, I think, of over hundreds of them. Industrial activities from making rubber tires to communication systems and what not. You really can find all of them listed there. When my people in industry see this chart, they always find out where their corporation belongs to.

APPENDIX

1977 US National Input-Output Tables
(millions of Dollars)

From \ To	Agriculture 1	Mining 2	Construction 3	Manufacturing 4	Trade & Transportation 5	Services 6	Other 7	Final Demands	Total Outputs
1 Agriculture	31930	29	934	63714	733	4745	172	27405	129663
2 Mining	275	5566	2397	77704	372	19794	1217	−29295	78031
3 Construction	1383	2923	303	8706	7182	32058	4971	206809	264334
4 Manufacturing	26198	7425	98337	517408	30714	93117	3301	578483	1354983
5 Trade & Transportation	7463	1726	29970	88262	30795	27283	2274	323867	510639
6 Services	13056	9875	20934	101615	90507	188910	6226	648710	1079832
7 Other	355	292	844	10375	4050	9521	492	220583	246512

Direct Requirements

From \ To	1	2	3	4	5	6	7
1 Agriculture	.2463	.0004	.0035	.0470	.0014	.0044	.0007
2 Mining	.0021	.0713	.0091	.0573	.0007	.0183	.0049
3 Construction	.0107	.0375	.0011	.0064	.0141	.0297	.0202
4 Manufacturing	.2020	.0952	.3720	.3819	.0601	.0862	.0134
5 Trade & Transportation	.0576	.0221	.1096	.0651	.0603	.0253	.0092
6 Services	.1007	.1265	.0792	.0750	.1772	.1749	.0253
7 Other	.0027	.0037	.0032	.0077	.0079	.0088	.0020

Total Requirements

From \ To	1	2	3	4	5	6	7
1 Agriculture	1.3608	.0170	.0491	.1097	.0139	.0213	.0042
2 Mining	.0404	1.0963	.0567	.1119	.0163	.0389	.0092
3 Construction	.0289	.0500	1.0172	.0253	.0250	.0415	.0224
4 Manufacturing	.5162	.2391	.6841	1.7307	.1637	.2191	.0456
5 Trade & Transportation	.1303	.0549	.1762	.1384	1.0867	.0562	.0172
6 Services	.2503	.2088	.2127	.2206	.2553	1.2570	.0426
7 Other	.0112	.0084	.0121	.0171	.0123	.0136	1.0030

Source: Miller, R & Blair, P., Input-Output Analysis, Prentice, 1985

On the upper part of the chart—and you have a condensed version of it in the appendix to my document, my statement. It's very condensed, but there you can see that there are seven sectors there, a hundred sectors there, and when we really go to work, we can use 1,200 sectors. In other words, you really speak completely.

Each row of figures shows the output of a particular industry—for example, the steel industry. The last figure on the right shows the total output of that industry in a particular year. All the figures, each figure in the row, shows how much of steel was delivered, the steel industry itself, the automobile industry and so on. Very often the steel industry uses this to find out where does the steel go, including exports.

Now, we look at the same figures—take, for example, a column, the automobile industry. One figure shows how much steel we use. Another says how much plastics we use. A third one says how much of a particular type of machinery we use. A third says how much power—in other words, on the bottom you can see how much skilled workers were used, how much unskilled labor.

When I tried to explain to my friend what this is, I say that technology is a cooking recipe. When a housewife wants to bake a loaf of bread, she looks in the cookbook for the technology of making bread. She knows how much different ingredients goes into it. Here we have the ingredients of what goes into each sector of the American economy, including the service industry, banking. It's a very concrete operation determined by the technology.

Now, if in any given moment of time you use technology—sometimes you use two different technologies at the same time—for example, electric energy you can produce by thermal plant, by hydro, and atomic. For each, of course, you can have your distinction. What does it make possible for us? We produce the final output, we deliver to final users different commodities. To produce it, all industries must operate. Some of them really don't produce for final users. The steel industry produces only something for hardware stores, but we are still contributing directly by going into the production of other goods and so on.

Now, it would be terribly complicated just trying to figure out. Fortunately, you can set up a system of equations, which is rather simple. When you have this cooking recipe to determine on what level each industry operates, since we know how much technology, we determine how much labor we use. If you introduce information, you have five different cooking recipes. So this gives you a picture of the economy to produce.

Now, what is technological change? Essentially, it is a change in the cooking recipe. If you change your cooking recipe, usually we try to change acceptable technology as such which will we say reduce the cost. In other words, it will enable us to produce the same output with less input, or, of course, we can turn it around and produce the same input with larger output.

If you look at our history, we have input-output charts for 1920, and so on. Some countries have longer. You can see how technological change enables us, and you do it in detail. In other words, I always say we don't work with inferences. We try to work with evidence. There is a great difference. Unfortunately, too much of economics is inference. Anyhow, it works.

I produced the first input-output table in 1931. Everybody said it's quite impossible. I produced it. Now, over 30 large countries produce them, over 120 countries make them now. It's a gigantic investment. But it is fact. I could never do it. But I showed them that it can be done, so we spend the money and I use it, and they use it.

One of the countries which did it best is Japan. Here I want to show you, these three volumes are the Japanese input-output table for 1 year. We're doing two. This single volume is the American input-output table. You see the difference? We have 20 people collecting it and they have 230 people. And they have a Cabinet Minister supervising it. It is a gigantic work in which they put great resources, and then make their computation on the basis of that. We construct scenarios to introduce technical change or endeavor. What effect will it have on the economy compared with the facts.

We keep the whole economy in mind, but also we get all the details, which is why I always said I would like to have a bunch of engineers helping me. I would like to hire Battelle, but since 1 hour of engineering consulting costs $200, I couldn't possibly do it.

Now, my friends from ASME came and said, "Look here—" they came to me. They want to help because they think it can be done. In the Department of Commerce, of course, we have a division which does it. We do a splendid job despite the short resources.

The next step we need, when you have a cooking recipe, is not only a description of flour and milk, and so on, you need pots and pans, ovens and buildings. This is capital. If you want to expand your production, you must build a plant. Capital has very often been described in sophisticated language that nobody understands. But actually, it is essentially goods in a particular combination. So we should have similar tables of not only how much is current input and labor, but how much of different types of equipment produced by other industries. This gives us really a rather—I won't say complete, but a sufficient description of the technology to understand two things: What investment does it take and capital to increase output, and how is it related to other industries which produce with capital and must supply the current inputs.

Now, this is what the so-called input-output analysis is all about. I do not want to go further in technicalities, but one is terribly important. To get these figures, you cannot simply speak in terms of total labor force, total national income. Technology is kind of a mystical thing. You must be able to incorporate specific details because this is where your decisions are made and this is where industry makes the decisions to introduce new technology. Costing out of technology essentially in our free economy is the principle on which you decide to introduce it or not.

Now, it is easy to see. I give you here a physical relationship. You produce so much steel and use so many ingredients. But to justify it, you must balance your money for your costs. You sell steel at a certain price; you buy all the other things at their prices; you pay wages to the workers for this, and you must have a return on capital for the use of capital. I don't call it interest rates. It's even more. I mean, very often industry wouldn't touch investment on this unless they can make about 20 percent on capital investment.

Now you see that you must equate what you receive now in money. You have another system of equations which reflects really the technologies, too, indirectly. The price really reflects technology because it is determined by your cost, which is independent of the prices, and the amount of different commodities. So we deal both with prices and physical amounts, and you must keep account of both of them. So we need here two sets of coefficients to describe technology, current inputs and capital stocks.

It is an operation which requires very large amount of capital formation. We have, of course, national accounts. We have beautiful national accounts which put things in dollars, but usually very aggregated figures. It gives you total national income or total investment. Now, these are not in terms in which you can describe things. If you know these details, of course, you can conclude your national account, and today a large part of resources on the basis of which our income division are really input-output tables. If there is a discrepancy, it is usually on national accounts, not input-output tables.

You cannot go on the street and meet national incomes. You cannot ask a layman what is national income. But I can go to engineers and ask how do you make steel, and what are the different methods of not only now producing steel but 5 years from now, at least, automobiles already on the drafting boards—even alternative methods of producing automobiles are being considered. In other words, we can begin to construct a scenario of where the economy will be not now but, 10 years from now. But if we can introduce technology based on another technology, which is what the Japanese do, we might be able to do it as well as they do.

I happened to have been asked by the Italian Government for the development of a transportation system, railroads, highways, air traffic, and waterways, with whom they work, industries. I got the technology involved and then put it together.

The first input-output tables were constructed from the bottom up. We first get the total flows and then get these ratios, but more and more we go the other way around. We first get these technical ratios and then we compute the total flows, depending on what outputs you want to get. In the case of environmental disruption, for example, which is on the chart—let's not forget, with dirty water which comes out, they are on the chart because they are produced by industry. Elimination of pollution is on industry, and it's on the chart, too—not on this chart. Much more detail.

This happens to be for illustrative purposes, published by the Scientific American magazine. We have already published a couple of them.

So this is the way in which you do it. Later there will be a seminar someplace here in which I explain in detail, but, for example, if you have confronted industry with two technologies, your choice will depend very much on pricing, what the wage rates are, and what the return is which you must have on the capital.

If you want a very high return on capital, you will not introduce too many technologies. They are terribly expensive. But it happens, but not always, that modern technology requires more capital and gets a little less weight in the decision process.

I can show you a chart, which I don't have here, which shows that if you imagine you want to earn 40 percent on your capital, you introduce new technology in relatively few industries. But once they are satisfied with 20, 15, 12 percent and down, what interesting results come from these computations.

Two of the biggest industries in our country are education, including research and health care. I made my first computation and an assumption that in Government everything is financed simply on the market, selling products. Imagine our health care was done simply by private corporations without any subsidy. We would not introduce scanners and new technology. It would be too expensive. We couldn't find people who are ready to pay directly. Why introduce it there? Because these industries are financed by transfer of funds. They recover the costs not only from revenue but also by having sudsidies, which is justified. Of course, obviously our military establishments are completely subsidized. By the way, the Pentagon is one part of our Government who relies on input-output analysis to a very great extent. To achieve economies in the Pentagon, they always work with input-output analysis.

Now, I think I should stop and only mention one last thing. It is not cheap, not cheap at all. You cannot do it on the back of an envelope. It is big teamwork, and we do it already. We have already the Department of Commerce. The Japanese have a much larger one. But what I say in research, if you really get results, which people can understand as well, it is usually good. Even high cost justifies it. If you just have them do a little study everywhere, you save on expenses but your result is zero.

I am arguing—and I was very pleased that my friend understood. Let us do it, to seriously invest funds and do it. I frighten some people, and I don't mean it seriously. If you spend the price of a modern, single attack bomber on this research, we can begin to catch up with the Japanese and the Germans. Otherwise, we will just be researching and working in circles. This is my argument.

Now, before I stop I would like to introduce the gentleman which contacted me, who came as—he is Won Chow, the technical director of the American Society of Mechanical Engineers. You can see in the record what other people see. He is a computer scientist, an engineer at MIT, and he also began as a violinist.

(Laughter.)

He is a man who designed the guidance system for the Atlas missile. He understands technology and he understands what it means.

I leave, if time is left, to answering questions. Please, if you have some questions which I can answer—if I cannot, I say I cannot; if I can answer, I would be delighted to respond. I left purposefully some of these questions which might arise in your minds to answer.

[The prepared statement of Dr. Leontief follows:]

REPORT BEFORE THE COMMITTEE ON SCIENCE AND TECHNOLOGY
U.S. HOUSE OF REPRESENTATIVES
Science Policy Task Force
Hearing on
"Research Funding as an Investment"
April 30, 1986

The Next Step In Expanding The Input-Output Data Base

by
Wassily Leontief

Mr. Chairman and Members of the Task Force:

It is a pleasure for me to be here today to take part in the discussions and respond to the questions of your committee. I concur with Dr. Ali Seireg in his statement that Engineering Economics is a crucial need and this is the time to address it. Engineering Economics requires a very close relationship between engineers and economists. Its consequences can be very far reaching indeed. Most important to your committee, its outputs may be able to satisfy your needs to measure the potential return on our investment in research programs. I am submitting a written statement for your reference. However, I wish to devote the precious time at the hearing to answer your specific questions and to discuss your concerns.

In addition to commenting in general terms on the challenging questions addressed by this Committee, I will describe in my written opening statement a systematic fact-finding approach that is already being used by government and business, in this country and other countries, to reach a better understanding and quantitative assessment of the contributions that specific technological advances introduced in various sectors of the national economy play in improving its performance in general and, in particular, its competative position in international markets.

The large chessboard-like table displayed before you is really a picture of the U.S. economy. It describes the flows of goods and services -- including labor services -- between its many different sectors.

The figure shown at the end of each row represents the output of a particular sector -- say, the steel industry -- named on the left hand side; the numbers entered along a row indicate how the total output of that sector was allocated among all other sectors -- so many tons or dollars worth of steel were delivered to the construction industry, so many to makers of office furniture, so many to the automobile industry and so on.

The same entries examined column by column show the amounts of goods and services produced by different sectors that each sector listed at the top of the table has absorbed -- as its input -- in order to be able to produce its output. In other words, the figures entered in each column reflect the input structure of a sector.

Being essentially technologically determined, the structure has to change whenever the method of production, that is the technology, used by the sector is replaced by another.

The first official U.S. input-output table for the year 1939 was published in 1941. In some thirty countries official input-output tables are being published regularly and in many other countries they are being compiled on a less regular basis. While Japan seems to lead both in the collection and practical utilization of these data, the United States -- with much smaller teams assigned to that task, one in the Bureau of Economic Analysis, the other in the Bureau of Labor Statistics -- is not only holding its own but is slowly catching up.

The peculiar contribution of input-output analysis consists in slowly but systematically narrowing the gap between micro- and macro-analysis. It has often been said that in economics one must make a choice between detailed description of individual trees or aggregative description of the entire forest. Input-output analysis is an application of the modern systems approach that permits us to describe the forest in terms of individual trees. It provides the means for observing and analyzing simultaneously the quantitative relationships between hundreds and even thousands of variables while preserving throughout all the operations the identity, the name and the address, of each of them. In this way, the power of modern computers permits us to describe the structure and analyze the operation of ever larger systems.

In the process of data gathering, the shift toward a more and more disaggregative approach is bound to require greater emphasis on qualitative as contrasted to purely quantitative differences and on qualitative as distinguishable from purely quantitative change. Much ingenuity has been used, particularly in constructing long time series, to secure comparability -- or should one say commensurability -- where it actually does not exist.

Quantitative comparability can always be secured at the price of suppressing qualitative differences by means of aggregation. For instance, an aggregative time series describing the long-run changes in the level of output of "building materials" might refer to brick and lumber in the early years, cover cement and steel in its middle stretches, and a great variety of nonferrous metals and plastics in the recent years. The aggregative approach is particularly ill-suited to the description of a modern economy experiencing rapid structural change. To analyze its structure and its operation, it is more important to describe concisely the interdependence between its different component parts than to trace separately the history of each of them, a history that turns out to be in most instances relatively short. In the design of the requisite data bases, if one has to make a choice between the two, a crosssectional approach seems to be relatively more important than a longitudinal one. Compilation of disaggregated national input-output tables represents one of the most ambitious statistical implementations of the cross-sectional approach.

Detailed description of intersectoral flows of goods and services contributes greatly to the understanding of the structural and, in particular, the technological relationships that control the interdependence among the different sectors of our economy. The picture will, however, remain incomplete so long as the description of current flows has not been supplemented by an equally detailed description of the capital stock requirements of all producing and some of the more important final consuming sectors. In modelling the role of capital goods in the production process, input-output analysis follows what might be called the low road, in contrast to the abstract, mainly theoretical high road approach which shaped to some extent the aggregative treatment of capital and capital formation in national accounts.

In speaking of cooking recipes rather than production functions, I intend to stress concrete description of directly observable processes in terms usually employed by those who operate them or have designed them. A minimal, stripped down, input-output version of a technological cooking recipe does not tell how to operate a steel mill or run a railroad, but it provides in condensed, somewhat stylized form, quantitative information indispensible for measurement of the economic interdependence among the many different sectors of a complex modern economy, explanation of the choice among alternative technologies and, what is particularly important, systematic assessment of the economic implications of technological change.

That information is provided in the form of two sets of technical coefficients. (1) Current input coefficients -- now usually derived from input-output tables -- describe the amounts of various materials, power, various types of labor, and other current inputs needed for the regular maintenance of fixed equipment. Each such coefficient describes the number of units of a given input which are absorbed by the sector using that input to produce one unit of that sector's finished output. (2) Capital coefficients describe stocks of various types: building machinery, auxiliary equipment and inventories of both raw materials and goods in process that the particular technological recipe specifies as the amounts needed for transforming the current input (as described by the corresponding set of current input coefficients) into the finished end product. For purposes of structural analysis, capital coefficients measure the stocks required per unit of their rated full capacity output.

The two complementary sets of technical coefficients constitute a comprehensive data base that, if employed within the framework of a static or dynamic input-output model, can advance a direct realistic approach -- as contrasted to a speculative one -- to the formation and solution of a great variety of structural economic problems. To give an example: with proper sets of current input and capital coefficients put in place, the so called "dual" system of input-output equations can be employed as a tool for "costing out" alternative technologies. Taking into account the interdependence among the different sectors, this approach can show to what extent the choice made in one industry can affect the outcome of a similar contest in another industry.

A recent pilot study involving the systematic application of that approach demonstrated, in terms of concrete numerical results, to what extent the introduction of automation and computerization in different U.S. industries will depend upon, or possibly determine, the level of real wages and the rate of return expected or rather required to be earned on the invested capital.

The construction of a comprehensive table of capital coefficients should be given high priority as a logical next step in the development of a comprehensive database required for systematic description and, to some extent, anticipation of structural changes that can be expected to transform most sectors of the American economy in the future even more rapidly than they did in the past.

This, for obvious reasons, will be a much more difficult task than that of securing systematic estimates of the corresponding flow-coefficients. I do remember, however, that fifty years ago when I proposed the compilation of the first input-output table -- that of the United States for the year 1919 -- the Research Committee composed of senior professors of the Harvard Department of Economics advised me not to embark on such an impossible task.

The early estimates of the capital requirements of U.S. industries were based on ingenious or, should one say, imaginative, combinations of aggregative figures and partial information contained in special tabulations presented in various U.S. Census volumes, with data gathered from a great variety of corporate and industrial reports.

An excellent source of detailed technical information was the so-called "Certificates of Necessity," which were construction licenses issued by the War Production Board during the last World War. Each contained a detailed listing of building structures, machinery and auxiliary equipment that went into the construction of new plants and into major capacity expansions of already existing ones. This information covered a great variety of plants located in many different regions of the country.

Forty years passed, and although some scattered tentative estimates of the capital coefficients for many different industries exist, no efforts to collect and publish estimates for all sectors represented in the large disaggregated input-output charts have yet been initiated, at least not in this country. (Statistics Canada has assigned the task of constructing such a comprehensive data base to its Structural Analysis section organized several years ago.) Without entering into discussion of technical details, I will now submit to your attention a few fundamental observations that might be taken into account in laying out a plan for carrying out that task.

1. Systematically organized sets of flow and stock coefficients placed within the framework of a detailed, disaggregated input-output model provide the indispensable but also sufficient factual information needed for systematic understanding of economic -- as contrasted to purely engineering -- aspects of technological change.

Some of the private commercial information services can provide a very large amount of miscellaneous financial and technical data on a great number of firms operating in all sectors of the U.S. economy. However, the collection of such information for private purposes without a specific analytical focus can not be expected to obviate the need for compilation of the structural data bank described above.

2. While measurement of essentially physical quantities in terms of fixed baseyear prices -- a standard practice in construction of most input-output tables -- is non-objectionable from a formal theoretical point of view, the use of physical units -- for example of KWH's in measuring energy -- has proved to be more convenient, even unavoidable in cases where the primary source of information happens to be of a technical engineering nature.

This procedure, although perfectly acceptable in theory, raises the brows of a national accountant who expects the same dollar total entered at the bottom of each column of an input table describing all transactions in terms of current prices, to be equal to the value of the total output of the same sectors entered on the right hand side of the corresponding row. Since all figures entered in the main body of an input-output table, even those measured in dollars, can, as has been explained above, be interpreted as representing physical amounts of different inputs each measured in appropriate but different units, the column totals cannot necessarily be assigned an economic meaning. (How does one add, say, KWH's of energy to tons of steel?) They can be expected to equal the corresponding row totals only if all physical amounts can be assumed to be valuated at current ("equilibrium") prices. For purposes of computational convenience, a table, as well as all inputs and stock coefficients related to it, can, however, be calibrated by multiplying all original entries (irrespective of whether they represent dollars or some physical quantities) by appropriate equilibrium prices. These can be easily computed (on the basis of the original non-calibrated table) through solution of the so-called dual set of inputoutput equations describing the relationship between a given set value-added coefficient and prices, a solution that equates the value of the total output of each sector to the aggregate costs of inputs and the so-called value-added payments incurred in the process of producing it.

I dwell at some length on the description of that calibration procedure since it provides a method of translating the engineering language usually employed in concrete description of actually observed production (as well as many, if not all, consumption processes) into the terminology employed in describing the input-output structure of an economic system, computing the numerical values of the parameters employed in modelling it.

3. The measurement of stocks and of stock-flow relationships which enters into detailed description of the input-output structure of a multisectoral economy differs significantly from those measurements derived from compilations of standard national accounts.

The foremost concern of a national, as well as that of a private, accountant is the comparability of balance sheets describing the state of the same economy or business at different points in time. Such comparability is secured by the application of internally consistent, but essentially arbitrary, aggregative, accounting procedures that actually render those balance sheets incomparable because qualitatively different objects cannot be said to be comparable. When it comes to measuring stocks of capital goods in describing the process of technological change, an accounting procedure that valuates them indirectly by cumulating past increments over a long period of years can not be deemed to be satisfactory, at least not satisfactory for this particular purpose.

A more direct approach would involve sampling surveys aimed at simultaneous evaluation of both the current input and the capital coefficients for groups of production units stratified by the types of technology they employ. Emphasis should possibly be placed on covering in these surveys recently built or re-equipped production units that, as a rule, incorporate technology of the latest vintage.

Sampling surveys have from the outset played a significant role in the compilation of input-output tables. What is proposed here is a systematic extension of that role.

Although the suggestions here might at first appear to be an advance into new territory, what is actually proposed represents the fulfillment of responsibilities with which the U.S. Census Bureau was originally charged when it was established more than two hundred years ago. The early census volumes, which were first published by the Treasury and later by the

Department of Commerce, contained, in addition to statistical tabulation, long introductory surveys of progress in different branches of industry and agriculture with detailed descriptions of newly introduced technologies, sources of principal materials, and distribution of finished products.

In its analytical aspects, input-output analysis differs from other approaches in its ability to make fullest possible use of detailed factual information. Moreover, in constructing a data bank including such information great stress is laid on the use of direct evidence, as opposed to indirect inference which plays a greater and greater role in the construction of standard econometric models.

Thus it is not surprising that input-output analysis is much more expensive tha econometric research, not to speak of the deductive fully theoretical approach based on acceptance of mathematically convenient but quite arbitrary factual assumptions.

Input-output analysis is expensive because it is very labor intensive. The construction of a detailed input-output table involves the use of a large number of specialists assessing expert knowledge. This type of research has to be carried out by a team, each member of which has to have acquired not only first hand knowledge of the economic and technological relationships that have to be observed, but also a full command of the theoretical, mostly mathematical, tools employed to explain them. In deciding whether the value of the output of this large-scale operation can justify its cost, we must also take into account the fact that the young, budding economists -- mostly post-graduate, graduate and sometimes even undergraduate students -- acquire on the job a much more realistic grasp of the structure and operational characteristics of the economic system than they can acquire attending standard econometrics courses and studying textbooks on micro- and macro- theory.

APPENDIX

1977 US National Input-Output Tables
(millions of Dollars)

From \ To	Agriculture 1	Mining 2	Construction 3	Manufacturing 4	Trade & Transportation 5	Services 6	Other 7	Final Demands	Total Outputs
1 Agriculture	31930	29	934	63714	733	4745	172	27405	129663
2 Mining	275	5566	2397	77704	372	19794	1217	−29295	78031
3 Construction	1383	2923	303	8706	7182	32058	4971	206809	264334
4 Manufacturing	26198	7425	98337	517408	30714	93117	3301	578483	1354983
5 Trade & Transportation	7463	1726	23970	88262	30795	27283	2274	323867	510639
6 Services	13056	9875	20934	101615	90507	188910	6226	648710	1079832
7 Other	355	292	844	10375	4050	9521	492	220583	246512

Direct Requirements

From \ To	1	2	3	4	5	6	7
1 Agriculture	.2463	.0004	.0035	.0470	.0014	.0044	.0007
2 Mining	.0021	.0713	.0091	.0573	.0007	.0183	.0049
3 Construction	.0107	.0375	.0011	.0064	.0141	.0297	.0202
4 Manufacturing	.2020	.0952	.3720	.3819	.0601	.0862	.0134
5 Trade & Transportation	.0576	.0221	.1096	.0651	.0603	.0253	.0092
6 Services	.1007	.1265	.0792	.0750	.1772	.1749	.0253
7 Other	.0027	.0037	.0032	.0077	.0079	.0088	.0020

Total Requirements

From \ To	1	2	3	4	5	6	7
1 Agriculture	1.3608	.0170	.0491	.1097	.0139	.0213	.0042
2 Mining	.0404	1.0963	.0567	.1119	.0163	.0389	.0092
3 Construction	.0289	.0500	1.0172	.0253	.0250	.0415	.0224
4 Manufacturing	.5162	.2391	.6841	1.7307	.1637	.2191	.0456
5 Trade & Transportation	.1303	.0549	.1762	.1384	1.0867	.0562	.0172
6 Services	.2503	.2088	.2127	.2206	.2553	1.2570	.0426
7 Other	.0112	.0084	.0121	.0171	.0123	.0136	1.0030

Source: Miller, R & Blair, P., Input-Output Analysis, Prentice, 1985

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May 8, 1986

The Honorable Don Fuqua
Chairman, Committee on Science
and Technology
U.S. House of Representatives
Washington, D.C. 20515

Dear Mr. Fuqua:

After taking part in the hearing held by your Committee on Wednesday, April 30, I and the represenatives of the American Society of Mechanical Engineers had an opportunity to discuss the proposal described in my written statement with staff members of yours and several other House and Senate Committees as well as with the top administrators of the National Science Foundation. The results of these interesting exchanges prompt me to suggest that the following paragraphs be added at the end of my statement:

> The high cost of collecting and organizing the technological information required constituted up to now a principal obstacle to the full fledged application of the so called input-output approach to the systematic assessment of contributions that the introduction of new goods and services and new methods of production can make to strengthening the competitive position of the U.S. economy.
>
> The American Society of Mechanical Engineers has now removed that obstacle by offering the full active cooperation of all of its thirty five different technical divisions in the collection of that indispensable information within the framework of a newly established Technical Economics Initiative. Considering the high cost of specialized engineering consultation, the monetary value of that contribution will certainly amount to several million dollars. The voluntary nature of that offer, made by a leading professional organization, introduces into the formulation and practical implementation of the proposal described above an entirely new, element — that of private initiative. In the course of his testimony Dr. Ali Seireg has already expressed expectation that, following the ASME example, other similar associations with interest in science and engineering might be interested in joining the ambitious enterprise.

We have good reason to believe that the National Science Foundation will be prepared to provide finanical support for further development of the theoretical foundations and methodological procedures indispensable for accelerated advance in any rapidly developing line of scientific inquiry.

One remaining bottleneck, will, however, still have to be removed: the newly constructed, massive data base will have of course, to be incorporated in the existing system of government statistics, more specifically, in the input-output tables of the U.S. economy which are being periodically compiled and published by the Input-Output unit of the Bureau of Economic Analysis in the Department of Commerce. Budgetary limitations combined with the freeze imposed on the replacement of personnel who are retiring or leaving will make it quite impossible for that unit to accept the new information and to incorporate it systematically and effectively into its own data bank. The critical bottleneck can be removed only by the introduction of budgetary arrangements which would permit that unit to add a certain number of additional skilled professionals to its present staff.

Mr. Chairman, I would like to express the hope that you and your colleagues on the Committee will find it fit to recommend that such budgetary and personnel arrangements be introduced with minimum delay. We in this country have discussed for long enough the general philosophical principle of proper methods to provide sufficient support for research and development activities which will enable this country to retain its leadership in economic growth and social advancement. It is high time that we begin to act.

Sincerely,

Wassily Leontief

Wassily Leontief

WL:sc

PANEL DISCUSSION

Mr. **Fuqua**. Thank you very much, sir.

As you concluded at the end, this is very expensive, very complex and complicated, probably lengthy. How could it contribute to be usable in the day-to-day operations of, say, the decisionmakers in Congress?

Dr. **Leontief**. You saw already some examples done by way of Battelle. Studying with technologies, as Dr. Seireg said, on what happened because of corrosion of equipment. He concluded, that if you can use modern existing corrosion techniques, you can increase our national income by 4 percent. At the present time we grow about 3 percent. Never mind. Make only one-tenth of that. And this is concrete. You just see what has to be done and it will be done by industry if you tell them what can be done.

Here is our last volume [indicating]. It is called "Future Impact of Automation" of our labor force. A different question, but the outcome is exactly the same way. We use the present input-output methods, and when we constructed a new one which introduce the technologies which are already known but not yet introduced, we came out with a result for short periods of time, which the National Science Foundation asked us to do only to the end the research, tomorrow, practically. Our result was, I must say, rather optimistic for a short time. We will not expect, which is what labor forces worry about, technological employment in the sense of what number of jobs will go down. But the structure of the labor force will change.

One interesting result is, for example, that the number of ordinary office workers, and what is called middle managers, will become less important——

Mr. **Fuqua**. Let me pursue another thought along that line. Suppose that a problem was identified. How long would it take—the turnaround time—before some action could be taken to correct or adjust for that problem? Say something in foreign trade, or if you're seeing an issue develop, what is the turnaround time identifiable before corrective action can be taken?

Dr. **Leontief**. How we can solve specific problems, which is your question?

Mr. **Fuqua**. Well, I'm really saying specific problems, but once the specific problem is identified, is there a delay time before the identification of that problem would be reflected in the system?

Dr. **Leontief**. Oh, yes; now, in Japan, they construct an input-output table in 3 years. It takes us 9 years to construct. Of course, we have such a small staff. This explains the cost. We use big computers. I use the biggest computer and it's very cheap. We speak of the computer age, but it is information which counts, not computers. It's like here. If you use 20 people instead of 200 people, the budget will increase.

Now, one nice thing is, my friends in the Society of Mechanical Engineers have agreed to help me. They will give me advice. Remember, ASME has 35 technical divisions. They are interested, and they are anxious to help, and it will not cost the Government anything. Yet this assistance is with millions of dollars.

Once we begin to gather the data, but we need help from the Government. For example, obviously you must strengthen the units in the Government that have to integrate with information.

At the present time, only 20 people work only on construction input, and every time somebody resigns, there is no replacement. It will take us 20 years. This is, you see, what is involved in this work.

One more remark. We work by teams. I began in Harvard where sometimes we employed 30 people. The people who worked with me receive the best training in economics we have. An ordinary student in economics studies microeconomics and macroeconomics and what not, he forgets it when he goes on the real jobs. Honestly, he writes speeches for the chairman of the board of his corporation, or becomes a professor. Our people, by the time they're through, know industries. These are very desirable people to work in Government, and input-output helps them.

Let me tell you a small anecdote. Once when the administration changed in the past, there was a lot of input-output work done, but it was on a primitive level, on the Council of Economic Advisors. At that time it was too primitive, but helpful. And then it was abolished. The staff members, the working staff members, the researchers, did it under the table. They didn't tell the bosses they were doing it, but they asked me to send them figures to show how to do it. It was kind of a "black market" in input-output. You know that a black market means it's a very desirable commodity, particularly for research. [Laughter.]

Other questions, if you have them.

Mr. FUQUA. Dr. Seireg.

Dr. SEIREG. I would like to just add to your concerns, Mr. Chairman. We are a volunteer society, as you know, in the ASME, and we have access to massive amounts of distinguished technical people and data that money cannot buy, actually.

We are, however, used to using 40,000 volunteers sometimes to do standards in the public interest. So I think the collaboration of engineering and economics is the key item here, as evidenced by the fact that we are testifying as a team. We hope that this effort will bear results for everybody's interest.

I believe it does not take very long to get, once the model is constructed, even in a crude manner, to get information that is useful, to make long-range decisions on investments and science and technology, for Congress, for industry, or for universities for that matter.

Mr. FUQUA. Gentlemen, thank you very much. I wish we had more time. Time seems to be the evil of all of us.

Dr. LEONTIEF. May I just add one thing. To work on the competitiveness of American products in the foreign markets, of course, we know the foreign countries' technology, so we can see what is happening.

Mr. FUQUA. Thank you, gentlemen, very much.

Dr. SEIREG. Thank you very much.

Mr. FUQUA. Our next witness will be Dr. Richard N. Foster, director of McKinsey & Co. Dr. Foster, we are very pleased to have you this morning. We will make your statement part of the record, if you wish to summarize.

[A biographical sketch of Dr. Foster follows:]

RICHARD N. FOSTER

Richard Foster is a director of McKinsey & Company, the international management consulting firm. He works with the executives of some of America's most successful companies, as well as consulting with companies in Europe and Japan. He heads McKinsey' worldwide practice on the problems of managing technology.

Dr. Foster is the author of "Innovation: The Attacker's Advantage" (Summit Books, April 1986). He has also written articles in Harvard Business Review, the Wall Street Journal, Business Horizons, Research Management and Technology in Society.

He earned his B.A., M.A. and Ph.D. degrees in engineering from Yale University. Before joining McKinsey in 1973, Dr. Foster was with Union Carbide where he was the international product/market manager for fine chemicals.

He serves on the board of Oak Ridge National Laboratory and has served on committees for both the National Academy of Engineering and the National Academy of Sciences. He is an adviser to the Industrial Research Institute.

STATEMENT OF DR. RICHARD N. FOSTER, DIRECTOR, McKINSEY & CO., NEW YORK, NY

Dr. FOSTER. Thank you, Mr. Chairman. I will try to make my formal statement as brief as I can.

My experience has been largely with industry, so that the comments that I am going to make today, and the ideas that I am going to reflect, have been developed on that basis.

The problems that you pose about the benefits of research for society, and particularly how that knowledge can help in the allocation of Federal funds for science and technology, is not a question that I spent a lot of time thinking about. The only portion that I and my colleagues at McKinsey really spent a lot of time thinking about is the industrial portion. I will try to present our observations about the practices of American industry today from that perspective, and from the perspective of the chief executive officer of the corporation who also, from time to time, has to allocate some resources.

Having looked at industry over a 15-year period, the bottom line is that industry, by and large, has not found it attractive to systematically and quantitatively measure the benefits of its investments in research and development.

I think there are a couple of reasons for this. The first reason is research and development are two different things, whose benefits have to be measured in different ways. Second, the standards for comparison of those benefits are not at all clear. These two problems I think have stopped industry from its efforts to measure the benefits of research. Some progress has been made, in particular with some work that the Industrial Research Institute has been doing along with us, to try and understand the way in which research and development contribute to society.

Let me draw your attention to the first chart, where I will discuss the return on money spent for the development. In industry, of course, we can think about the benefits of research as profits to the corporation, and we can think about an index of the ratio of the benefits of research profits, to the amount of money that has been spent—the research investment. That is the resource, of course, that we're allocating.

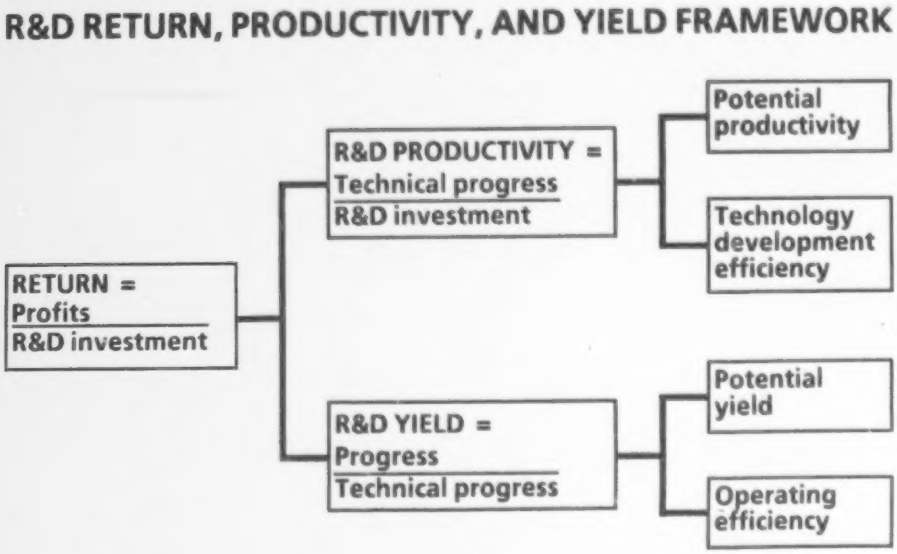
R&D RETURN, PRODUCTIVITY, AND YIELD FRAMEWORK
RETURN = Profits / R&D investment
R&D PRODUCTIVITY = Technical progress / R&D investment
R&D YIELD = Progress / Technical progress
Potential productivity
Technology development efficiency
Potential yield
Operating efficiency

CHART 1

We can think about this ratio as being the result of two other ratios. The first is what we call R&D productivity. This is the technical progress that one makes for an investment in R&D. It is the productivity of the labs in the same way that productivity of a steel mill would be measured by the tons of steel produced per hour of labor put in. It is not profitability, per se, but it is output of the plant. Therefore, we need a second ratio, which is the amount of profit we make for the technical progress we put in. Or, in our steel mill analogy, if we make an extra ton of steel, what we can sell it for.

We know that in cases of high demand for steel, extra productivity is worth a great deal. We know that in cases of low demand for steel, extra productivity is not worth a great deal. So separating out these two aspects of the total return, R&D productivity and R&D yield, seems to us to be an important thing to do. It is important because the principles which underlie R&D productivity, and the principles which underlie yield are very different. We think that R&D productivity is determined by the nature of the way in which people develop different technologies. If we focus on the key creative resource in R&D, the designer of new products, and trace the amount of result he gets for a given amount of effort put in, it will trace this S-shaped curve that we see here (chart 2).

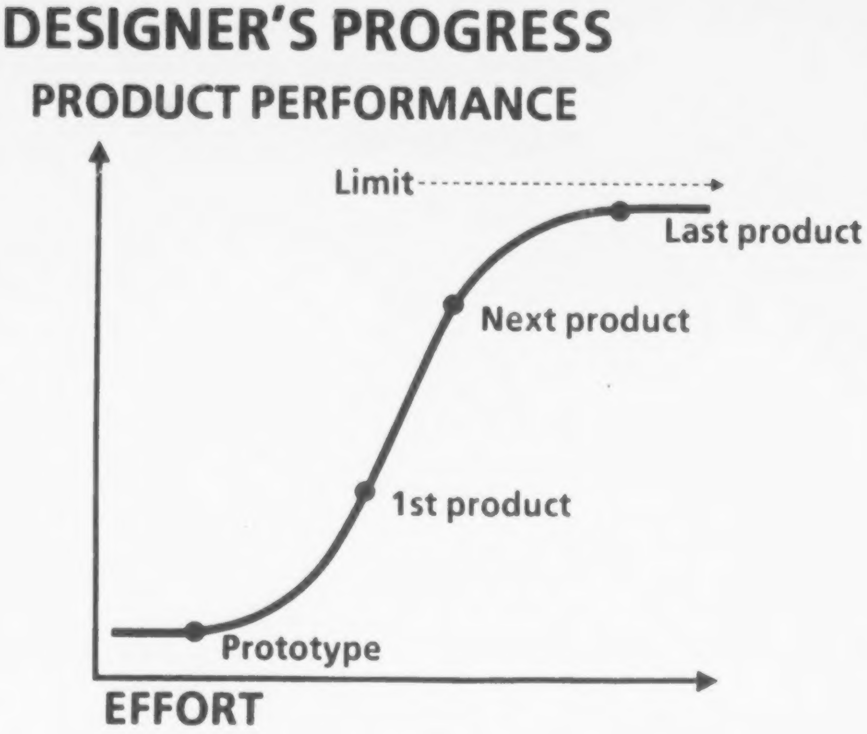

Chart 2

In the beginning, as the ideas are fairly unclear about what is going to happen in the designer's mind, he puts in a lot of effort and makes very little progress. Then as he gets a key design idea, he can make very rapid progress with a little bit of effort, producing the first product, the second product, and the third product in the generation. Then, as he begins to approach the natural limits of what that technology can do—the ultimate productivity of the steel plant, the ultimate speed of a computer, the ultimate speed of an airplane—the designer has to put in more and more effort in order to make progress.

Now, we can think about this chart as we used in the steel mill example. This might be the chart of the tons of steel the worker produced by putting in effort. The productivity of that worker is really the slope of this curve at any point on the chart. If we could see the next chart here, please (chart 3).

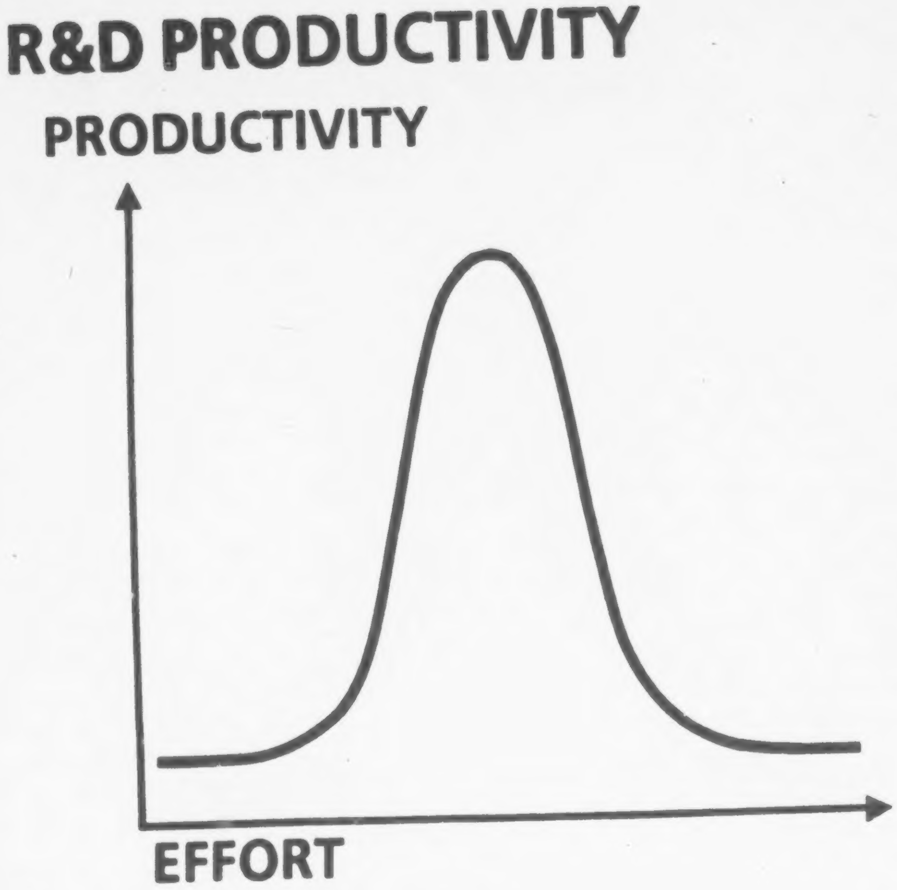

Chart 3

Because the designer's efforts start off as unproductive and then increase to very high levels of productivity, and then fall off again as the limits are approached, his productivity does the same thing. That is to say, in the beginning stages of a technology, productivity will be very low; it reaches a very high state in the middle of the history of the technology, and then declines dramatically.

We have measured these curves quantitatively in some of our industrial work and have found that the peak on that curve can easily be 10 times that of the baseline. That is to say, one company pursuing a technology that was near to its limits might have to spend 10 times the amount of money to get the same technical progress as one who is investing in the middle stages of the technology.

What all that means is that the choice of technology to be pursued is far more important than the precise measurement of the productivity of the researcher. The variations in productivity among researchers or designers, in the case that we have used here, can easily be a factor of 10 to 1, and in electronics we have seen that to be 20 or 30 to 1.

Efforts to precisely measure this are, I really think, irrelevant. The point is to pick the technology that has the most potential, the one that is furthest away from its limits.

Now, referring to the first chart again, to get to the total return, we then have to switch our attention to the other side of the equation, the R&D yield—that is, the amount of profit that is made from that technological progress. These profits are not determined by the principles of the S curve but they are determined by the principles of microeconomics, laws of supply and demand and competition. There is nothing in those laws that guarantees the technological progress will be beneficial for anyone. There are certainly cases where we have introduced products into this economy which are advanced technologically but provide no useful service for anyone. We also know of industries, such as the electronics industry in the last few years, where even though the benefits have been provided in denser and denser computer chips, the competition has been so intense that not only have those companies not made any money, they have lost money.

So there is nothing inherent about the yield side which tells us that precise measurement is going to be helpful to us on that side, either. The key in these cases is to pick the technology that has the most technological potential and has the most utility to the end user. If that job is done well, then the returns will follow and precise measurements of the return will, I think, be seen to have reasonably little value for the allocation of resources within an industrial corporation. It is for that reason, I believe, that most corporations do not really attempt to systematically measure the benefits of their research and development spending. They recognize that efforts spent there are not going to be nearly as productive as efforts spent to evaluate the technical potential and the value of these technologies once in place. Measurement after the fact is not helpful.

Now, the extent to which this applies to the Government I'm not qualified to say, although it does seem to me that we have in the Government the same circumstances that we have in industry; that is, we have communities of providers of technology in the various departments, and we have users of that technology which may or may not be in that department or, indeed, under the aegis of the Government at all.

Those users do perceive benefits from these new technologies and, therefore, we can look to those users for the measures of benefits, whether they be economic measures or not. We can look to the providing agencies for measures of technological progress and R&D productivity and, therefore, the debate in Congress might well focus on the measures of productivity. What does R&D productivity mean for various agencies, what technologies are being pursued, how far from the limits are those technologies today, and what benefits will those alternative technologies have for the end user populations? In that way, through that dialog, which I believe is more precise than attempts to measure the productivity or the returns on research than by looking at patents, Nobel prizes and so on, I believe the debate will be more tightly focused.

In that debate, I would urge Congress to take a look at international comparisons, what we are doing with respect to our chief competitors, because there are no absolute standards of adequacy for these measures of productivity in return. There are only relative standards. For example, I have learned recently that, for the first time, in 1985 the Japanese have sold a greater dollar value of licensing contracts than they have bought, so for the first time now Japan is licensing more technology—licensing out—than they are licensing in. This is when they are spending roughly a third on R&D of what we spend in this country. Something's out of whack there. We don't need precise measurement to tell us that. We do need to focus the debate on the technologies that we're pursuing and to begin to focus that debate on the technical potential.

That completes my formal comments here. I would be happy to take any questions if you have them.

[The prepared statement of Dr. Foster follows:]

Richard N. Foster
McKinsey & Company, Inc.
April 30, 1986

TESTIMONY FOR
SCIENCE POLICY TASK FORCE HEARING
ON RESEARCH FUNDING AS AN INVESTMENT

Congressman Fuqua and Gentlemen of the Subcommitee: I am pleased to be here with you today to share my views on the measurement of the benefits of research for our society. As you have pointed out in your documents, research and development (R&D) provides benefits for many areas of our society. The economic health of the industrial sector, its associated consumer sector, military strength, the health of our population, and our university teachers and students are all certainly important objectives for our R&D. However, I will confine my remarks to the benefits of R&D for industry and thus, indirectly, the consumers. That is where I've spent my career. I will not take up your time with my profound thoughts about the benefits of R&D for the military or health or the university. Perhaps you will find my comments on industry useful in thinking about those areas at some later date.

MEASURING THE BENEFITS OF R&D IN INDUSTRY

As I think back over my industrial experience in the past 20 years, I think it is fair to say that I know of no systematic and sustained effort to measure the benefits of R&D in any company that I have known. I have worked with 30 or 40 companies, mostly in the top 200, and I have known many more informally through the Industrial Research Institute (IRI). That is not to say there are not periodic examinations. There are, but there is no sustained and systematic effort of which I am aware.

The reasons for this lack of effort to measure the benefits of R&D in industry probably are twofold. First, research and development are not really the same thing, they are two different things: research, which I think of as the acquiring of knowledge, and development, which I think of as the application of knowledge. As such, they must be measured and evaluated differently, using different methods and different standards. Second, there is not yet agreement about what the standards should be. So these are two confusing factors for most industrial companies. Some progress is being made, however, and I would like to review it with you today. Let's look first at development.

Development

Perhaps the easiest way to evaluate development is to think about the conventional industrial measure of benefits, profit. Specifically I want to think about the ratio of the profits received by the corporation, divided by the investment in R&D that has been made. This is a sort of generalized benefit/cost index. It is a good measure of the overall effectiveness of development efforts.

One can break this ratio into the product of two other ratios, each of which have significance. (Summarized in Improving the Return on Research and Development, IRI, 1984, submitted.) (See Exhibit: "R&D Return = Productivity times Yield.) The first is the ratio of technical progress made by developers for the amount of work they do. For example, if they were working on the development of a more fuel-efficient automobile, the technical progress would measure the improvement of fuel efficiency. Dividing this progress by the amount of money or manpower it took to achieve those improvements would provide a measure of the "productivity" of the R&D effort. This measure of productivity is analogous to the manufacturing productivity measures used in the national accounts. It is productivity in the same sense that the productivity of the steel industry is determined by counting the number of tons of steel produced and dividing it by the manpower required to produce that steel. Of course, we can also talk of capital productivity and the measure of that would be the tons of steel produced per dollar of capital invested. Of course, we can combine labor and capital to compute the "total factor" productivity. These same conceptual underpinnings can be applied to the measure of development productivity that I have just defined.

Development productivity alone, however, does not get us the return ratio we are seeking. To get that return ratio one must multiply the productivity by a second ratio, which is the ratio of profits received for the technical progress achieved. In other words, how much money was made as a consequence of improving the automobile engine efficiency. This ratio we will call the "yield."

The reason for breaking apart the return ratio, which is the objective we are seeking, into two parts is that the principles underlying the two subsidiary ratios, productivity and yield, are quite different. Productivity in the sense that I have defined it here is a function of the efficiency and effectiveness of the technical efforts. It is independent of the surrounding economic context in which the progress takes place. Yield, on the other hand, is a function of the economic context. It is a function of the extent of demand and supply, the utility to the customer of improving the technology, the degree of competition, the legal web surrounding the industry, and the collective strategies of the actual and potential players in the market place.

For example, in the automobile example, even if one increases the efficiency of the engine by a substantial amount it is no guarantee that one will make a profit. The profit that one makes from the advance depends on many factors other than the extent of the advance. It depends on the:

¶ Strategies of the Japanese

¶ The extent of which the improvements are protectable

¶ Whether the price of oil is high or low since that determines the value of the advance to the consumer.

In other words, it depends on the standard range of competitive and customer inputs.

The total return equals productivity times yield. That is to say, in practical terms, we invest effort and money to achieve technical results (productivity). These results, which I have called technical progress, then enable one to make money or to transfer benefits to the end user (yield). There is no guarantee that profits will be made because of the nature of competition and of the uncertain usefulness of the results to the users. This two-part model then breaks apart the technological aspects of benefit creation from the economic aspects. Thus, understanding productivity and yield are key to understanding the total payoffs or benefits from development.

Qualitatively, the conceptual framework has been found to be quite useful for a wide variety of companies in the Industrial Research Institute. (See "Improving the Return on Research and Development, Industrial Research Institute, 1984, submitted.) Using the framework quantitatively is made more difficult but has been done from time to time, when the nature of the decision to be made warrants the extensive data collection effort. Perhaps the key to the successful quantitative application of the model is being able to define a meaningful measure of technical progress. Our measure of engine efficiency is only one of several measures of engine performance. Reliability and cost are others. Each measure of performance, or attribute, will have its own "productivity." When one is working on improving an engine, one tries to improve all of the attributes simultaneously, not simply one at a time. So, figuring out what portion of the investment belongs to each one of the attributes is a difficult, if not impossible task conceptually. It is often easier to combine the individual attributes into an overall performance index and then examine the progress of this index with investment than it is to split out the investment associated with each attribute. The index method has been used, for example, in tire cords, as documented in the attached papers.* Let me examine productivity and yield in more depth.

1. Productivity. Over the years we have learned that there is a typical pattern of productivity which is governed by something that we call the S-curve. It is the classic pattern of learning and delearning that a designer will go through when designing a product (Exhibit: Designer's Progress).

In the beginning, the designer makes slow progress because he does not understand the nature of his objective and how closely he can meet it through the application of his efforts. As he discovers the answer to this riddle he makes very rapid progress, improving his design quickly until he hits a point of diminishing returns, a point at which he has solved all easily solvable problems and only has the more difficult ones before him. At that point, his progress slows down despite his experience. The effort that is required to make further progress increases asymptotically as the limit, or ultimate performance of the technology, is approached.

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* "In the case of the tire cords for bias-ply passenger tires, technical performance is defined in terms of the combined parameters used by the B.F. Goodrich Company in evaluating suppliers' offerings. These include tensile strength, abrasion and corrosion resistance, and adhesion to the rubber carcass of the tire--all factors related to desired end-product attributes, such as dimensional stability, longevity and cost. The cumulative R&D effort usually can be measured in man-years of R&D effort, or, as it is in this example, in dollars of R&D investment." Timing Technological Transitions," by Richard N. Foster, Technology in Society, Vol. 7, pp. 127-141, 1985.

If instead of tracking the total amount of effort that the designer puts in we track his productivity by tracking the amount of effort he has to put in to make a bit of technological progress (or in other words we look at the slope of the S-curve), we can see that the productivity traces a bubble-shaped curve. It starts from a low level, increases to a very high level and then falls off as he focuses on more difficult problems. (See Exhibit: R&D Productivity Curve.)

We have measured these productivity curves and find that, unlike productivity variations in manufacturing which are often in the range of 0-15%, productivity variations in development can vary over an order of magnitude. In other words, the productivity in the mid-stage or most productive stage of the designer's experience can be 10 times or 1,000% more than at the beginning or end stages of the technology development.

To put this observation in practical terms, a company in the midstage of its S-curve can be getting 10 times the technical progress per dollar spent than the company pursuing development at the top of the S-curve. The message here is that precise measurement of development productivity is not important. What is important is being sure one has selected the right S-curve to pursue. Thus one does not need to measure technical progress precisely, if one has selected the underlying technological approach properly.

Trying to measure productivity quantitatively to choose between two approaches on the basis of differences misses the point. One can do a better job, with less effort, by examining the limits of alternative approaches and the benefits that would be brought to the consumer at those limits. This perhaps is the most fundamental reason industry has not spent a great deal of time trying to precisely measure development productivity. Precise measurement of the productivity does not make any difference.

Before I leave you with the impression that the only thing that makes a difference in development productivity is the choice of which technical area to pursue, I would like to soften my statement to say that the slope of the S-curve, or the development productivity is a function of several things beyond the basic technology chosen. These things include:

- The adequacy of the knowledge base available to the designer
- His ability to communicate with other designers with similar problems so that experience can be shared
- The time available to design the product (crash projects cost money)
- The critical mass of skills needed for effective design.

Differences of up to 30 to 40% can be found between best and worst competitors, due to these factors. These differences are certainly significant but not nearly as significant as the 1,000% difference that can be found due to technology choice. The point is that differences in productivity that would be revealed by precise measurement are totally washed out by the magnitude of the improvements that are possible. In productivity measurement rough measures are accurate enough for the decisions of resource allocation that have to be made.

2. Yield. Yield measures the economic benefits of technological change. It is not governed by principles as straight-forward as the S-curve. It is governed by the overall principles of supply and demand. As such, yield is determined by the structure of the supply curve, the demand curve, the extent of nature of competition within the industry, and the extent to which benefits accrue to the producer. I have covered the basic principles of yield in the attached papers. Suffice to say, there are two situations in which improvements in technology will not result in benefits accruing to the producer, or developer, of the technology.

§ No value to the customer. There is nothing which says that improving the technology will bring additional benefits to the consumer. In the early days of detergents, optical brighteners were added to detergents to compensate for the fiber roughness in the cloth created during the washing process. This roughness gave a gray-look to the fabric even though it was quite clean. Adding a small amount of optical brightener to the detergent improved the perceived fabric whiteness and brought benefits to the customer (cleaner fabrics) and to the developer (sales or profits) of the optical brighteners. Addition of more optical brightener further brightened the fabric and brought further benefits to the customers and developers of the optical brighteners. At some point, however, the fabric was as bright as the consumer's eyes could measure, and the addition of more optical brighteners did not result in additional benefits for the producer because they did not result in additional benefits for the consumer. Higher levels of technical performance did not bring more benefits. We find the same phenomena, for example, in home computers where additional computer speed will not have value for the consumer. The same is true in pharmaceuticals where smaller and smaller doses will not result in easier and easier pill ingestion because pills are already at the smallest handleable level.

¶ Unprotectable technology. No benefits result to the producer for advancing a technology when a number of competitors can copy his approach to meeting the customers' needs. In this case, if the initial product is profitable it will be sufficiently attractive for new competitors to enter the market. They will be drawn into a cycle which will create overcapacity, driving prices and profits down. There is no sense in developing a technology one can not protect in some way.

No quantitative calculation of yield are necessary in these situations to understand yield. The result is known. Indeed, yield is rarely measured at all by industry, either before or after the fact. It is so difficult to attribute profits to technical progress *per se*, because other functional departments--such as manufacturing or marketing--have a claim on the profits too, that industry avoids the question of yield in favor of measuring return on investment. Industry does not try to break down the functional components of that return.

Thus companies have not found it beneficial to attempt quantitative measurement of the benefits, or return, from development. Rather they have focused on qualitative understanding of the productivity and yield factors which contribute to returns. This has been more useful to them.

RESEARCH

I have so far addressed my comments to development or knowledge application. I have yet to discuss knowledge acquisition or research.

The purpose of research in the eyes of most industrial research executives is to generate the knowledge needed to carry out design. As such, the measure of the success of research is not whether the information found leads directly to productive products (or processes), but rather whether the methodologies selected to carry out the research produce the information needed. Thus, the productivity of research (that is, the amount of information gained per unit of expenditure) is an entirely different notion than development productivity. I know of no company that has attempted to systematically measure research productivity because it is so difficult to specify in advance what information is needed. The best that is done in industry is the occasional post-audit. These audits often ask the question, "What did we get out of the research?" The "what" usually refers to useful products and processes as opposed to useful information. Consequently, it is not surprising to find that many of these audits conclude that not much of use has come out of research activities! This is the primary reason why central or basic research has been cut back in recent times in all but a handful of companies (IBM, GE, Du Pont, Ford, Xerox). But the wrong question had been asked. If the question had been properly framed a different answer would have resulted but finding that answer would still have been difficult.

Thus, R&D return, productivity and yield have rarely been quantitatively measured in industry. It is with this background in mind that I would like to comment on the transferability of industrial ideas (that is, don't try it) to the government sector.

APPLICATION TO GOVERNMENT ACTIVITY

There are similarities between technology development in the government and in industry, although the analogies are not very precise. In each case, there are providers of technology and users of technology. From a policy-making point of view, the notions of productivity and of yield are therefore applicable. Technical progress has to be defined. Measures of benefit would no longer be profit, or indeed even based on economic values. The measures would become specific for the area examined. For example, in the military the improved strike capability might be a measure of benefit. In the health care system, the number of people relieved of disease might be a measure of benefit. Thus, in principle, one should be able to find a way to apply the RPY model to the government. There are problems in the way of application, though. The RPY model has very rigorous standards for what information is needed, and most current attempts to use other indicators of R&D productivity, such as patents and Nobel prizes, fail the test because they fail to directly measure benefit. Indeed, they do not attempt to reflect benefits. In light of the RPY model, we can see that they are inadequate because they do not focus on an identifiable measure of technological progress that can easily be related to benefits.

Nobel prizes look at knowledge-building. They acknowledge that knowledge has been generated but do not tell us what progress has been made. In some way Nobel prizes are measures of scientific strength or potential, not return or benefit. They measure, if anything, the potential capability of the resource base that we have in place. While this is certainly important, it does not get at the issue at hand, measuring benefits. To measure the benefits one must somehow get at proper measures of technological performance.

Thus, measures like patents and Nobel prizes can be misleading. It would be better in my mind to forego the urge to be quantitative, in favor of the urge to be more precise about what it is we are trying to do with the science and technology enterprise. I would recommend first an effort to more precisely define the relevant measures of technological progress in each field. For example, strike distance for missiles. Second, we should make efforts to define what the benefits of that progress could be. No doubt these steps could represent a significant effort, but whether it is larger or smaller than the possible efforts to provide quantitative measures is arguable. Moroever, in the end this method would allow the government to make judgements about the relationship between the inputs to technology development and the benefits that flow from it. It should fall to the individual agencies to come up with their measures of technological progress, to identify the end user communities, and to seek ways of having the users identify net social benefit of technological progress.

Defining the measures of technological progress has been a difficult exercise for many companies. It has revealed that in many companies the marketing departments and technology-development departments have dissimilar views of the meaning of progress. In addition, performance measures of technological progress once defined do not necessarily stay constant. The emergence of new alternatives for solving problems, the changing social values of the country, changes in underlying economic-factor costs such as the price of oil, change the measures of performance. I would be surprised if similar problems were not uncovered in the government.

Nevertheless, you may find a certain appeal in the Return-productivity-yield model, as an aide to thinking through problems of resource allocation in the government. It does focus on the two key aspects of beneficial research and development, that is, the productivity of the efforts to generate the technology and the yield from that technological progress.

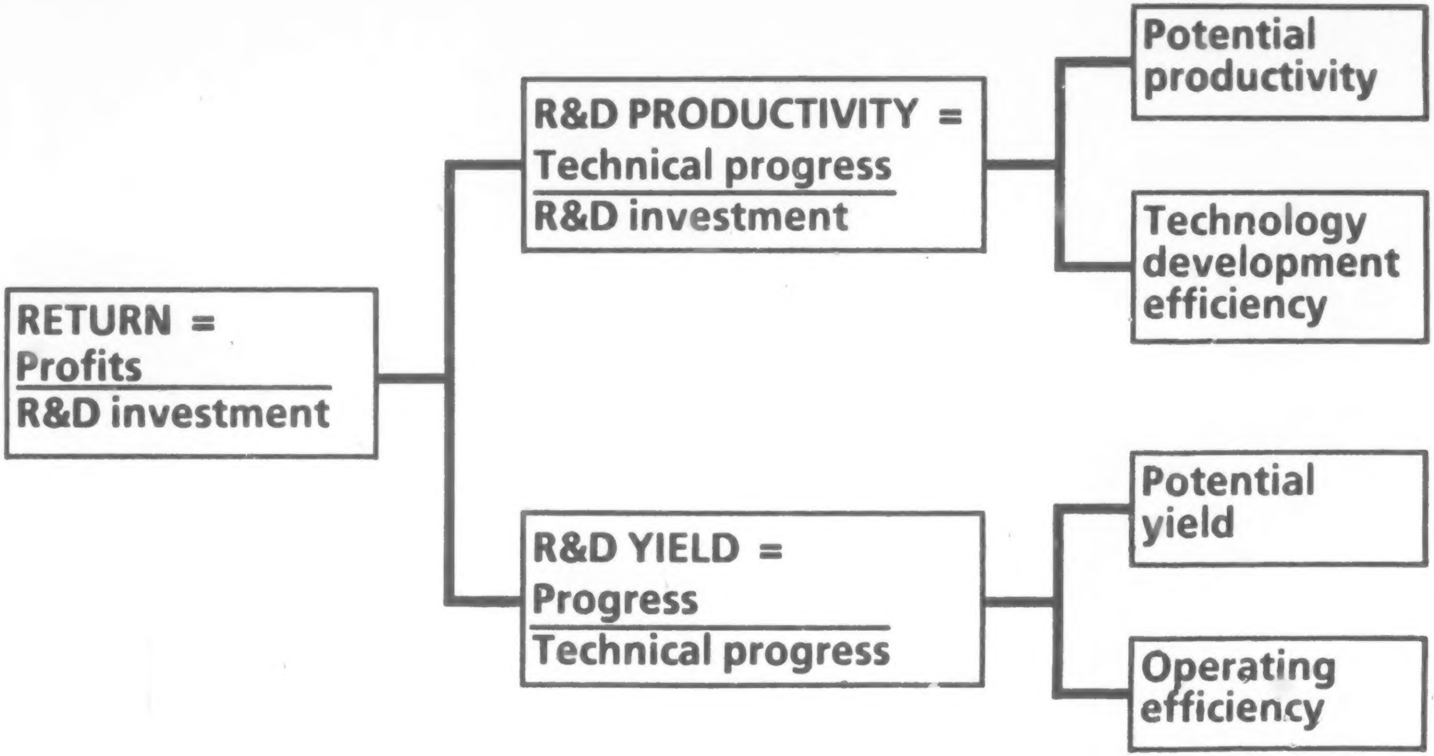
I would urge you to think through these measures of productivity and yield, not on an absolute basis but on a relative basis. After all, in setting priorities in government, as in industry, one needs to be alert to the competitors. This comparison will reveal major issues more clearly than will absolute measures. In this light, it may be interesting to note that for the first time, according to one source, the Japanese will sell more licensing contracts for technology this year than they will buy. Thus, they have a positive balance of payments in licensing agreements for the first time. This certainly should be a spur to us to do better. The point is, in government as in industry, we are looking at the relative productivity and yield of our efforts. International comparisons are an essential part of whatever performance measurement system we come up with.

* * *

I hope my message today has been simple and straight-forward. To the best of my knowledge, no systematic efforts are made in industry to routinely, quantitatively measure the benefits from investments in research and development, yet funds continue to increase. Despite many attempts over the years, quantification efforts have all failed for reasons that I have discussed. In our view, quantification is not as important as precision. It is important that we precisely think about what we are trying to get out of our technology investments rather than try to quantify the series of indicators that have less meaning. The return-productivity-yield model, I think, fits these conditions and could, with thought and effort, be usefully applied within the government to help think through issues of resource allocation.

Thank you very much for your attention. I am happy to take questions.

R&D RETURN, PRODUCTIVITY, AND YIELD FRAMEWORK



$$\text{RETURN} = \frac{\text{Profits}}{\text{R\&D investment}}$$

$$\text{R\&D PRODUCTIVITY} = \frac{\text{Technical progress}}{\text{R\&D investment}}$$

Potential productivity

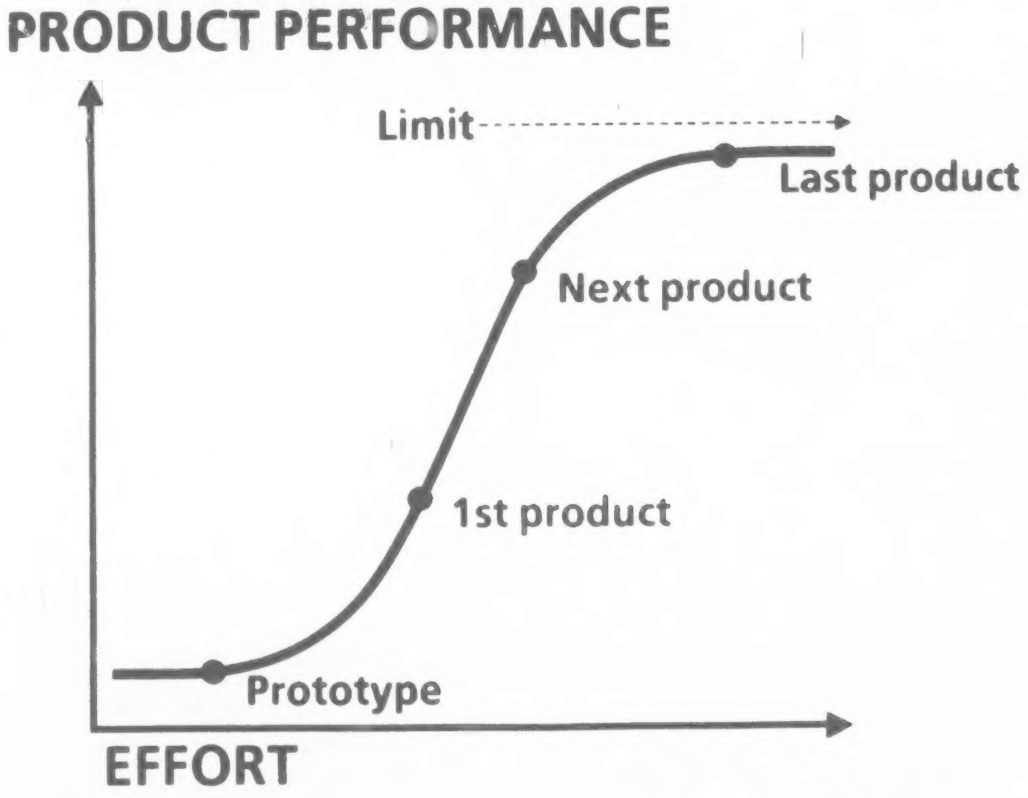
Technology development efficiency

$$\text{R\&D YIELD} = \frac{\text{Progress}}{\text{Technical progress}}$$

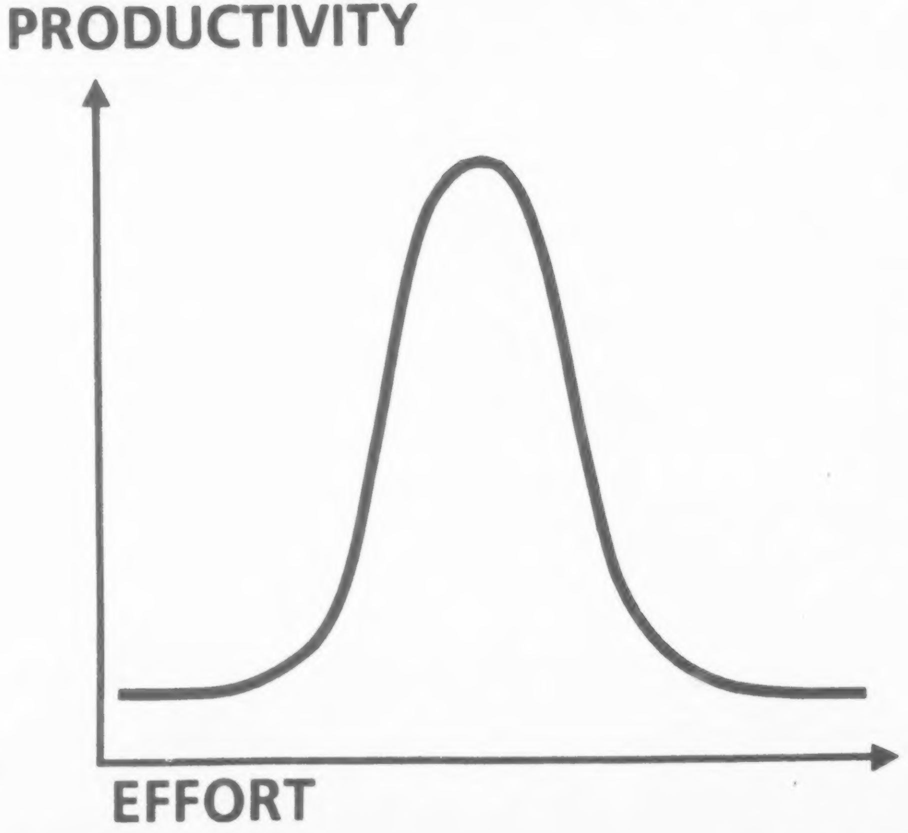
Potential yield

Operating efficiency

DESIGNER'S PROGRESS



R&D PRODUCTIVITY



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Timing Technological Transitions

Richard N. Foster

ABSTRACT. Technological discontinuities are among the most disruptive challenges facing corporations, and they are on the increase. Empirically, it appears that few corporations are able to survive discontinuities. They either lose market share, money or their independence. Despite the importance of the discontinuity problem for top management, few have done much to better anticipate and deal with these increasingly frequent events. They act as if they assume that there is nothing that can be done. The problem appears to be one of misplaced faith in some conventional assumptions about what it takes to succeed. The author examines these assumptions, particularly those dealing with the supposed advantages of defenders, and pinpoints their errors. This knowledge then leads to positive solutions that hopefully, in turn, will lead to increased success for some of our most important companies.

When the transistor replaced the vacuum tube in the mid-1950s, every one of the top ten producers of vacuum-tube components was caught flat-footed. Six of them discounted the importance of the new development and held aloof from solid-state technology, while the other four tried to change horses, but failed. Not one of the ten is a force in today's $10 billion semiconductor industry.

This familiar scenario is not confined to high-technology industries. Consider tire cords. Sixty years ago they were made of cotton. Then – in the mid-1940s – the lead shifted to rayon and the American Viscose Company became the leading supplier of cord. Early in the 1960s, however, Du Pont's nylon tire cords captured the top position. The lead shifted yet again in the late 1970s, when Celanese took control with its polyester fibers. Each time a new technology superseded the old, industry leadership changed hands.

Few companies, it seems, are able to make a successful transition to a profitable new technology. Typically, technology leaders wake up too late to the threat of a new technology.

How do technologies mature? Why is the process so often accompanied by management paralysis and competitive dislocation? And what steps can companies take to improve their ability to anticipate and manage transitions between technologies, reducing the chances of such dislocation in the face of future technological change? These are the questions that will be explored here.

Richard N. Foster is a Director of McKinsey & Company and heads the firm's Technology practice. He is the author of a book on successfully managing through technological discontinuities, which will be published in early 1986.

Technological Limits and Potential

Technological improvement in any field is ultimately limited by the laws of nature. The number of transistors that can be placed on a silicon chip is limited by the crystal structure of silicon, the ultimate strength of a fiber by the strength of its intermolecular bonds.

Most industries are far from these ultimate natural barriers, and much more likely to come up against practical technological limits. The efficiency of today's car engines, for instance, is limited by the ability of existing engine alloys to withstand heat; hence, researchers are investigating the possibility of using ceramics in their place.

The mission of research and development is to advance toward the limit—both practical and theoretical—of a particular technology. How much improvement is possible is determined by the difference between the current state of the art and the technical limit. This difference can be called the ***technical potential***. As the state of the art nears its technical limits, each increment of research effort produces less improvement since the most effective ideas presumably have already been used. Said another way, as the limits of technology are approached, the returns from further R&D diminish. Many companies don't understand where the limits of their technologies lie, and finding out can be a complicated business. But the payoff can be great.

In the late 1960s, for example, IBM investigated the limits of computer technology.[1] Thorough evaluation at the Watson Research Labs confirmed the prevalent assumption that these limits were ultimately set by the accuracy with which computer circuits could be printed by photolithography. It also disclosed, however, that the state of the art of component design, device design, and—most particularly—chip "package" design all posed more restrictive practical limits. To get around these limits, the scientists changed the existing design in what became known as the "multiple stacked array" concept on which IBM's 4300 Series computers were based—a major market success of the 1970s that would have been impossible without thorough understanding of technological limits.

The role of technology in corporate strategy is directly related to technological potential. If the potential is high—*i.e.*, if the state of the art is remote from the technical limit (a situation typical of "high-tech" industries)—technology is likely to be an important element in the strategy of the business. If the technological potential is low, then other business functions are likely to loom larger.

The S-Curve Phenomenon

Of necessity, all R&D work is carried out in the context of technical limits. Early in an R&D program, knowledge bases need to be built, lines of inquiry must be drawn and tested, and technical problems surfaced. Researchers need to investigate and discard unworkable approaches. Thus, until this knowledge has been acquired, the pace of progress toward technological limits is usually slow. But then it picks up, typically reaching a maximum when something like half the technical potential has been realized. At this point, the technology begins to be constrained by its lim-

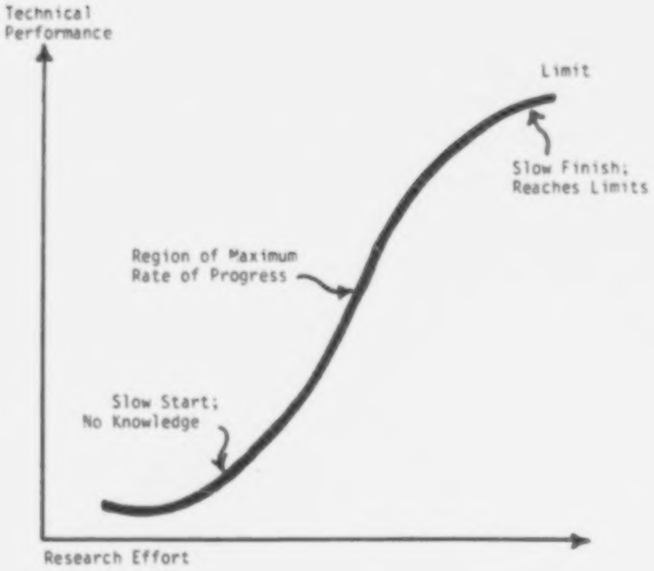


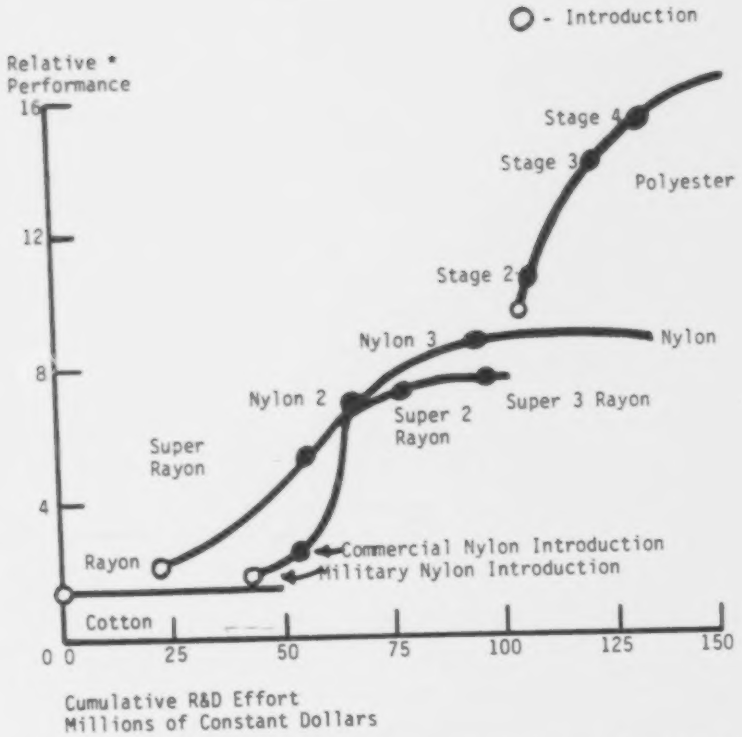
FIGURE 1. The S-curve phenomenon.

its, and the rate of performance improvement begins to slow. Diminishing returns set in. This phenomenon can be depicted as an S-curve. Figure 1 shows the concept schematically, while Figure 2 displays a representative set of actual S-curves from the tire cord industry.

In the case of the tire cords for bias-ply passenger tires, technical performance is defined in terms of the combined parameters used by the B.F. Goodrich Company in evaluating suppliers' offerings. These include tensile strength, abrasion and corrosion resistance, and adhesion to the rubber carcass of the tire—all factors related to desired end-product attributes, such as dimensional stability, longevity and cost. The cumulative R&D effort usually can be measured in man-years of R&D effort, or, as it is in this example, in dollars of R&D investment.

What is important to remember is that, no matter how vigorously a competitor backs its own technology, it cannot escape the realities of technical limits and the implications of the S-curve. Inevitably, the technology with the greatest potential takes control of the market. At the same time, competitive leadership is all too likely to change hands.

R&D productivity, like manufacturing productivity, may be defined as the ratio of the change in output to the change in input—*i.e.*, the improvement in the performance of the product or process divided by the incremental effort required. Accordingly, R&D productivity is determined by the relationship between technical progress and R&D investment at various stages of development. For example, Du Pont, pursuing a later stage advance in nylon, was getting 0.08 units of progress per $1 million of R&D investment. Its competitor, Celanese, then in the midstage



*An index used by tire cord customers to evaluate tire cord performance when compared to the performance of cotton, which has a performance rating of 1.0.

FIGURE 2. S-curves in tire cord technology
Source: McKinsey analysis.

of polyester development, was getting 0.33 units of improvement per $1 million of effort, four times as much as Du Pont.[2] Also, polyester tire cord was already superior to nylon, so Celanese was making faster progress on a better product. No company can withstand such competition for long. As Celanese's victory over the redoubtable Du Pont demonstrated, in technological contests the attacker often has a decisive economic advantage.

Sometimes, of course, market factors can actually cancel the day of reckoning. The end objective, after all, is not R&D productivity, but R&D return: the profit earned on the R&D investment. R&D productivity is only one of the two variables determining R&D return; the other is the profit made from a given technical advance. This variable, which can be called the *R&D yield*, is a function of the competitive structure of the industry: supply/demand conditions, relative power of customers and suppliers, company strategies, substitute products, external influences, and so on.

Decisions about which technology to work on and when to invest in it should be made on the basis of returns, and should involve both productivity and yield. Examining returns in terms of technological progress (productivity) and the economic value of that progress (yield) has organizational advantages, too. R&D productivity is determined largely by the R&D strategy and the efficiency of the R&D department in pursuing that strategy. Thus, controlling and improving R&D productivity is largely within the power of the R&D department. Yield, on the other hand, is the result of market conditions and competitor strategies, subjects that are the traditional responsibilities of the business units. R&D productivity and yield ultimately need to be coupled, of course, but this is best done after they have been examined independently.

Breaking down returns into productivity and yield also provides a more precise sense of the sources of low returns. For returns to be high, both productivity and yield need to be substantial. If either is low, their product will be low. If productivity is high and yield is low or negative, as it might be if there is a prospect for continued overcapacity in the industry, then returns will be low or negative. Accordingly, projects in this area should not be pursued, despite substantial technical potential and high productivity. Nor do projects make sense where yield is substantial, but the technology is mature. The low productivity of the mature technology indicates that little technical progress can be made. Investments in manufacturing, logistics, or marketing might be more productive. Companies that manage their technology well have recognized these pitfalls, and learned to avoid them.

The Impact of Discontinuities

By focusing R&D on improving the performance of current technology, companies limit themselves to evolutionary progress along a single S-curve until rapidly diminishing returns signal the imminent approach of technical limits. In failing to investigate the potential of new technologies, they often miss opportunities to exploit powerful competitive leverage.

Such opportunities, however, are not always easy to grasp. Almost by definition, the S-curves of different technologies are not linked. The gaps that separate them are often both substantial and strategically important, and to manage the transition is a difficult and delicate task.

The tire and cord technologies discussed previously, for example, are linked to bias-ply tires—a construction technology that has its own S-curve and its own technological potential, which happens to be significantly less than that of the newer radial construction. Goodyear clung to its bias-ply construction, which was beaten out by the Michelin-backed radials, just as Du Pont, sticking to nylon, was bested by Celanese. It is easier to detect a technological discontinuity from afar than it is when you are right in the middle of one.

One reason why it is so difficult to detect a technological discontinuity from the inside is that an existing competitor has more to lose with the introduction of a new technology, while a new entrant has little to lose and a great deal to gain. In tire cords, Du Pont felt it needed to protect its existing nylon plant assets. Celanese had no such need; it only felt a need to utilize its new polyester facilities. The need to protect its assets may have helped Du Pont believe that polyester had little market potential. History shows that this was wishful thinking.

This blindness to the advantages of new technology has affected almost all industries at one time or another. In electronics, the dramatic discontinuity seen in the shift from vacuum tubes to transistors was followed by a succession of further discontinuities as transistors gave way to SSI, to LSI, and then to microprocessors. In metal packaging, the move from three-piece to two-piece cans represents an important discontinuity (the retort pouch may be another). In each case, a technological discontinuity led to a competitive dislocation—and technology leaders became losers.

The essence of managing technology well, it seems, is the ability to make smooth and timely transitions to new technologies with superior performance improvement potential—in other words, to cross discontinuities effectively. In many cases, this ability may be the only key to the problems of declining R&D productivity and deteriorating competitive position. Superior operational efficiency in R&D can rarely, if ever, compensate for an inferior technical strategy.

Technological Myopia

Strategic errors, however, are not always to blame when a technology leader is overtaken by a competitor. Incorrect perceptions of technical limits, inability to measure technological progress, faulty interpretation of market signals, and unrealistic faith in "understanding customer needs" tend to mask the deterioration that sets in when an existing technology matures and the superior potential of an alternative begins to affect the competitive balance.

Managements unaccustomed to thinking in terms of technological limits and systematically measuring changes in technical performance almost always grossly overestimate the improvement potential of existing technology. It is tempting to assume that the rapid progress that occurs midway through the S-curve will continue and, unless technical performance *per se* is carefully monitored, the gap between reality and expectation is typically perceived too late—on a 10- or 20-year S-curve, perhaps five to seven years too late.

Knowing when to investigate new technologies is certainly important. On a symmetrical S-curve, half the potential for improving the technology typically remains to be exploited when R&D productivity has reached its maximum. At the midpoint, the bugs are out of the system, cash flow has turned positive, and the business has begun to make a decent profit. It may seem economically perverse to begin investing in a new technology when there is still a lot of potential left in the old. But since it can easily take five or 10 years to develop a new technology, this is often the best time to begin the move by shifting the focus of R&D.

Managers intent on lowering break-even points and improving profits are normally inclined to build bigger plants and spread their overhead over more products. They segment markets to squeeze maximum profit out of customer groups. They work to reduce fixed and overhead costs. All these measures help to maximize profits. And increasing profits tend, in turn, to justify further investment—especially when decisions are based on accounting and financial criteria alone. Only when aggregate financial measures have been broken down into their components, is it possible to distinguish between technological progress and economic returns, and gain real insight into the underlying forces at work.

While R&D concentrates on extracting marginal improvements from old technology, windows of opportunity open up for competitors. And, when a competitor does move in with a new technology, the company's response is typically to increase its investment in the old. But, in choosing to defend an existing technology, rather than displace it with a new one, management only increases the inherent economic advantage of the attacker by enabling him to develop his product or process unhampered by entrenched competitors.

When the first steamships were launched in the mid-19th century, established sailing-ship builders set about improving their design technology, and were building vessels that carried more sail, had faster hulls, and required smaller crews. But the result was only a brief reprieve for a mature technology, not an escape from the S-curve. Thanks to the far greater technological potential of steamships, sails eventually disappeared from the world's shipping lanes.

Sylvania, for another example, was hamstrung by this sailing-ship phenomenon when transistors made their commercial debut. Despite announced plans to move into solid-state technology, it kept on pouring R&D effort into increasingly sophisticated vacuum-tube designs, bringing out complex multi-functional units as late as 1968. Some evidence suggests that, despite Sylvania's public comments to the contrary, the company still believed that the vacuum-tube had a future. For example, Sylvania was an active member of the Electron Tube Information Committee, which was established to explain the advantages of vacuum-tubes and the disadvantages of solid-state devices to the public. In its efforts to wrest incremental performance improvement from its outmoded vacuum-tube technology, Sylvania got aboard the solid-state S-curve too late with too little effort to become a force in the new market.

Misreading Market Signals

The tendency to discount signs of market penetration by new competitors is another costly common error. New products are typically introduced in market niches where they can be highly competitive—niches apparently too small or insufficiently related to the core market to provoke an immediate competitive reaction from the industry leader. Yet, even in a growing market, when the defending producer's sales begin to level off, management often misreads this as a sign of predictable market maturation. And, as the substitution process gathers momentum, the company's sales actually begin to sag. Finally, as the challenger penetrates deeply into its heartland markets, sales collapse and profits swiftly deteriorate—almost before the defender realizes that the battle has begun. It is already too late.

Consider the pattern of substitution cases already mentioned. Polyester tire cords went from a 20 to an 80% share of the bias-ply tire market in 10 years. Radials repeated that market performance against bias-ply tires in four years, and steel tire cords took three years to go from 20 to 80% of the radial tire market. And, in electronics, transistors took seven years to displace vacuum tubes. In each case competitive leadership changed hands.

Part of the problem of recognizing a technological discontinuity is the problem of correctly defining the market. The more mature the product, the more narrowly the market tends to be defined. In the mid-1960s, all tires were bias-ply tires. Hence, by definition, tire-cord makers measured their market performance in terms of their share of the bias-ply tire market. When radial tires entered, the leading tire cord manufacturers should have redefined the market to include tires of any kind, rather than dismissing the radials as "niche fillers." Too many businessmen fail to recognize that any new product—and particularly one based on new technology—potentially redefines the served market.

A final strategic pitfall in technology management is management's tendency to wait for market research cues before committing funds to a new technology. There are three dangers in this perversion of the marketing concept. First, customers are rarely the best judges of potential utility of products that might require them to do things differently; change is painful. Second, the same market cues are available to all qualified suppliers, and the customer, once given the whip hand, can often play suppliers off against one another to advantage, as Boeing has learned to do in the aerospace industry with its materials suppliers. Third, sustained competitive success demands the ability to meet a market need in a unique way. In many businesses it is easy to copy a competitor's market research, but far less easy to copy its technology and harder still to copy its methods of developing new technology. Thus, a company is far more likely to win a real competitive edge by superior technological prowess if the technical potential exists than through any unique knowledge of market needs.

The Culture Trap

The most serious and most elusive impediments to effective technology management, however, are the fundamental cultural weaknesses that underlie the strategic errors being discussed.

A company's product and production technology is an integral part of its corporate culture. Technology directs and conditions management's intuitive strategic responses to opportunities ("can we make a profit on this?"). It conditions the assumptions on which the strategy of the business is based. When the technology changes, the whole corporate culture frequently must change as well. And this is a difficult and painful process.

For electrical engineers designing vacuum tubes in the mid-1950s, the transition to solid-state technology was a real culture shock. To design solid-state devices, they needed to learn an entirely new technical discipline involving a different mathematics. Expert vacuum-tube designers don't necessarily become good solid-state designers. But, because there were enough similarities between the two fields, some vacuum-tube producers assumed that making the transition would not be particularly difficult.

Sometimes a cultural preference for established technology is explicitly built into the management system; several major corporations still give top R&D priority to the defense of their existing product lines. The more doggedly they champion a policy of unyielding technological defense, the more vulnerable to new technology they become. Many R&D vice presidents, having earned their titles by successfully guiding their companies into new technologies, are not disposed to abandon their favorites easily. Indeed, they often inadvertently block the investigation of new and threatening technologies in the name of defending existing product lines.

Traditionally different industries have met technological challenges in different ways. Some observers see the emerging biochemical industry as a wave of the future. If they are correct, how well could established chemical manufacturers cope with the new technological environment? Consider just a few of the differences.

- The chemical business operates large plants whose capacities range from 100 million to one billion pounds a year. Biochemical plants might be small, with typical annual capacities of perhaps from one million to 100 million pounds.
- Whereas chemical plants usually produce a single product, biochemical products could be produced in multiproduct facilities.
- Chemical processes are typically high-temperature operations. Biochemical processes will probably take place at low temperatures, where the energy minimization experience of the chemical industry becomes much less relevant. Biochemical processes will require far tighter processes controls and heavier instrumentation than most chemical processes; thus capital requirements may well differ.
- Biological sterility, generally unnecessary in the chemical industry, is an important requirement in some biochemical processes.

Clearly, to capitalize on the opportunities in biochemicals, established chemical companies would have to do many things differently. As centralized operations become decentralized, strategies, as well as organizational structures, might need to be altered. Management systems might require revision with more authority given to people close to the market and the competition. With shrinking plant size, the role of the plant manager would diminish. New skills and staff needs would develop. New employees, coming from different colleges, universities and disciplines, would move in different cultural circles, and might even speak a different social language. All this would lead to changes in culture, shared values, and management style. The company emerging from such a metamorphosis might resemble its former self in little but name. This transition might be closer than some think. Presently about $1 billion is being spent on biotechnology R&D with perhaps 20% of this going to chemical applications. If the productivity of this work is five to ten times that of conventional chemical R&D, not an uncommon ratio in other transitions, then the total technical programs expected from biochemical research could be on a par with the technical programs expected from all other chemical research.

Cultural barriers to successful technological transitions are particularly difficult to overcome because they are qualitative rather than quantitative. Many managers are accustomed to dealing with strategic issues by creating new research programs or developing new measurement systems; few have experience in changing the entire nature of their companies. But nothing less than that may be called for.

Detecting Decay

Rather than wait passively for a competitive threat to develop, it is necessary for far-sighted management to head off technological decay by addressing the strategic and cultural issues as quickly as they can be detected and identified. A good way to begin is to assess the company's proximity to its technical limits. Typically, certain tell-tale symptoms tend to signal the onset of technological decay:

- *A feeling among top managers that the company's R&D productivity is declining.* Managements that don't measure technological progress have to rely on "gut feel" to know when the S-curve has flattened out. Unfortunately, by the time the flattening can be felt, the game may be lost.
- *A trend toward missed R&D deadlines.* Often interpreted as a sign that the R&D department is losing effectiveness, this may mean that the technology is approaching its limits, thus making performance improvement more difficult to achieve.
- *A shift in the company or industry from product- to process-oriented R&D.* As a technology approaches its limits, process improvements typically replace product improvements as the chief source of technological innovation. Many of our so-called "smokestack" industries are in this situation now. Paradoxically, the best corrective action is a move into unfamiliar product technologies or markets, a risky proposition at best.
- *An apparent loss of productivity in the R&D department.* There is no room for creativity in a technology that is bumping up against its limits.
- *Dissension among the R&D staff.* A particularly frustrating assignment—for example, closing a small gap between the "state of the art" and the technological limit—may turn scientists into pessimists. If the mood of the R&D people is bad, the limits of the technology are probably close.
- *A shift in the sources of sales growth toward narrower market segments.* As we have seen, the final stages of a typical substitution pattern are often dominated by product-line proliferation to meet the needs of small market segments. While this is usually a mandatory economic strategy, it does indicate increasing maturity, and should, therefore, be taken as a signal to look for new "S-curves" that could supersede the present technology.
- *A tendency for significant variations among competitors in R&D spending to produce ever less significant results.* When there is no market advantage to spending more, nor a market disadvantage to spending less, the technology has virtually reached its limits.
- *Dissatisfaction with the performance of a "new broom" R&D manager.* Sometimes a chief executive, frustrated at his company's declining R&D performance, will replace the senior executive with a new, more vibrant R&D manager who is expected to shake the department up and get it going again. He will fail too, if the technology is close to its limits.
- *A trend among smaller, weaker competitors in the industry to invest R&D effort in radical new approaches.* A small company, particularly if it is financially hard pressed, is more likely than an entrenched and profitable producer to invest in a really innovative solution to a customer's problem.

Toward Effective Action

Using these criteria to form an impressionistic assessment of the company's technological health can be an effective prelude to a thorough R&D audit. The audit itself should be based on the principle that R&D effort and R&D investment in a given technology should be proportional to potential for productivity and yield improvement. This principle allows the definition appropriate for R&D strategies (in terms of, say, aggressiveness, risk profile, time for commercial introduction, or a balance of work between old and new markets) to be used in a consistent way for each product line (Figures 3 and 4). These are then compared with actions being taken, or implicit strategies, and the inconsistencies identified. These become the agenda for reshaping the company's R&D program.

Audits of this kind are particularly useful to CEOs of companies with many product lines, since it is typically quite difficult to remember the details of hundreds or thousands of R&D projects. Accurate summaries based on these principles can be extremely useful tools.

These audits frequently reveal that companies are overinvesting in mature technologies and underinvesting in newer ones. In terms of the matrix in Figure 3, this means that companies putting too much emphasis on product lines warranting only "limited defensive support" are not putting enough effort into "heavy emphasis" product lines. To avoid this trap, companies generally will have to take action in two areas in addition to resource reallocations: (1) knowledge building, and (2) measurement and planning systems.

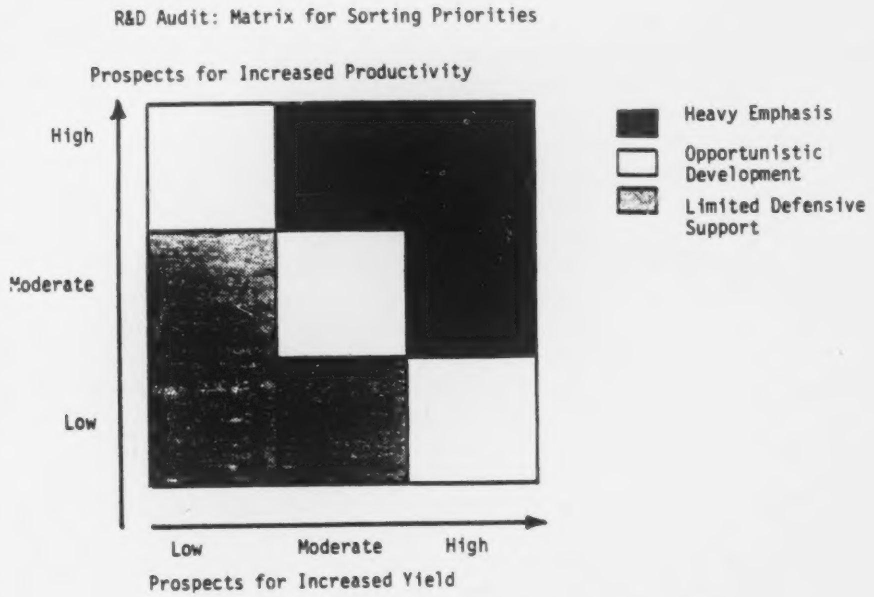

R&D Audit: Matrix for Sorting Priorities
Prospects for Increased Productivity
High
Moderate
Low
Low
Moderate
High
Prospects for Increased Yield
Heavy Emphasis
Opportunistic Development
Limited Defensive Support

FIGURE 3. R&D Audit: Matrix for Sorting Priorities

	Heavy Emphasis	Opportunistic Development	Limited/Defensive Support
Appropriate Funding	High	Moderate	Low
Expected Payoff	Long-term	Medium-term	Short-term
Acceptable Technical Task	High	Moderate	Low
Primary Focus of Work	Balance New & Existing	Existing Products	Existing Processes
Appropriate Level of Basic Research	High	Low	Very Low

FIGURE 4. R&D Audit: Typical Strategic Implications

Knowledge building. Since a major source of mistakes in technology resource allocation is inadequate knowledge about the limits of present and potential future technologies, efforts to correct this problem are obviously in order, but the utility of knowledge does not stop there. The new knowledge can also be useful in controlling the costs of development. A Rand Corporation study,[3] conducted for the US Department of Energy, for example, indicates that nearly 75% of cost overruns in the construction and operation of plants for new products and processes can be blamed on inadequate technical information.

Measurement and planning. Effective knowledge building is only the first step in improving returns from technical investments. The second step is to use this knowledge to plan better future investments. Armed with an understanding of the appropriate technical performance parameters, the limits alternative technical approaches place on these parameters, and an estimate of future R&D productivity based on analyses of past S-curves, senior management will be in a position to conduct a reasoned dialogue about the broad alternatives facing the company. In the course of this dialogue, existing assumptions grounded in conventional economic performance measures and incremental thinking are bound to be severely tested.

The ability to measure S-curve and R&D productivity can be integrated into the technical planning process to permit new assumptions to be tested. Then management can use these new technical information systems to determine which technology to pursue, when to pursue it, and at what levels of investment—and measure the results of these strategic decisions. Managers can determine whether a given effort has actually yielded the anticipated degree of performance improvement—and if not, why not. The conclusions, fed back into the technological planning and development process, can further refine its effectiveness. Such measures can rejuvenate a company.

Tuning in to Change

Radical technological change cuts away the company's base of technical and perhaps strategic intuition. Leading through the resulting maze of shifting organizational priorities requires new management priorities as well.

In most US corporations today, the CEO is the final arbiter of technological choice. It is he who finally decides to reallocate resources from old to new technologies. It is he who controls the pace of S-curve transitions, determining the rate of R&D funds transfer, the rate at which capital spending is shifted to new technologies, the redirection of marketing efforts—even the reallocation of managers.

The chief executive, in fact, *is* the corporate manager of technology. Unfortunately, the job is difficult. Confronted by an R&D program composed of dozens or hundreds of individual projects, small and large, the CEO is likely to be baffled by the task of assessing the relative value of each and relating it to the company's objectives, particularly in areas of rapid technological change. Yet if he cannot assure himself that R&D will be effectively focused on the technical alternatives with the greatest potential, he may be compromising the company's future.

The executive best qualified to aid the CEO is obviously the vice president in charge of R&D. But in too many US corporations this executive busies himself with more functional tasks—within boundaries implicitly set by the chief executive—than with basic issues of technological choice. In a recent Conference Board survey,[4] only one CEO in four named the R&D VP as a member of his inner circle. Something will need to change here. Until the R&D vice president earns the CEO's full confidence, strategic changes in technology will be painful, and future competitiveness in doubt.

Had such an executive been at Sylvania in the mid-1950s, he might have recruited more solid-state engineers. Were he in charge of R&D at a major chemical company today, he might be busy hiring biochemists, setting them up in an independent facility, and holding down investments in incremental improvement of conventional product or process technologies.

The R&D VP will need to become well tuned to overall competitive strategy and corporate options, and he may need to become more knowledgeable about corporate finance. He should establish a better working relationship with the top strategic planner and with the company's financial officers so that he can better empathize with their problems. In short, he will need to think like a CEO.

The Need to Anticipate

Since the shift to a new technology may take a decade or more to complete, most companies need to do a better job of anticipating technological development relatively far in advance. In short, they need a distant early warning capability.

In the mid-1930s, for example, Mervin Kelly, then research director of Bell Laboratories, projected telephone switching needs ahead to the early 1960s. He found that, with the current mechanical relay technology, an impossibly huge number of switches would be needed to support the future telephone network. This meant that a new technology would be needed. Kelly hypothesized that it might emerge from the new science of quantum mechanics. Accordingly, after World War II, he launched an R&D project to develop a quantum-mechanical amplifier. Bell Laboratories developed the first transistors in 1947; by 1955, transistors were an item of commerce. The whole process had taken 20 years (with 10 years out for the war).[5]

Development activities must be planned, funded, started, supported, and often completed before existing measurement systems can accurately predict their ultimate economic benefits for the company. But the rewards of anticipating future requirements are exceeded only by the costs of ignoring them.

One effective way a company can broaden its horizons is to push hard for the identification of technological alternatives. In the computer industry, for example, evidence is beginning to accumulate that solid-state components—designed and produced as they have been for the past 10 years—may be approaching the upper end of the S-curve. As the cost of creating ever more complex devices in the conventional way rises dramatically with each increment in performance, it is less and less economically feasible to persist with development at continually accelerating rates. Moreover, as the capacity of computer memories grows and the cost per bit of storage becomes infinitesimal, computer memories are becoming a commodity, and the market leverage of building bigger computers diminishes. This combination of increased expenditure needs and decreased leverage could create a scissors effect, constricting or even cutting off development. Already, in fact, the S-curve

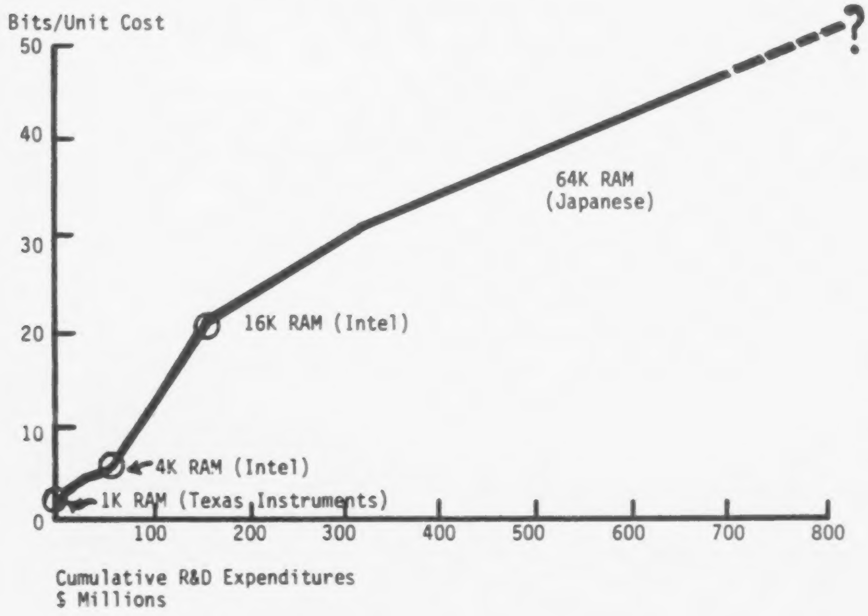


FIGURE 5. Topping Out in Electronic Hardware
Source: McKinsey analysis.

for random access memory chips (DRAMs) is exhibiting the classic phenomenon of diminishing returns (Figure 5), and many companies have already dropped out of the game.

As a result, it seems that the leverage in computer development is shifting from hardware to software, where no such diminishing returns are in sight. Monumental changes will accompany the transition. Manufacturing, always important in the computer hardware industry, may lose its place in the sun. Software manufacturers may already be beginning to embrace a strategy of creating only a few copies of each new design for distribution through telecommunications networks to the customers' own computers. With such an arrangement, the manufacturing process would take place, in a sense, on the customers' premises. The marketing manager would become a total business manager, controlling not only the sales of the product, but its manufacture and distribution as well.

Who would be most likely to survive such fundamental changes—the large hardware manufacturers, like IBM, Siemens, and Fujitsu, where manufacturing is an entrenched and powerful corporate function, or the smaller companies that now dominate the software industry? It may be hard to imagine anyone challenging IBM's preeminent position in the computer industry. But it was just as hard to imagine in 1955 that a little company called Texas Instruments would be a dominant force in electronics 10 years later. In the history of technological change, however, such revolutions are not rare. Companies that anticipate them, by continually investigating new alternatives and responding boldly to new challenges, are handsomely rewarded.

Notes

1. Robert W. Keyes, "Physical Limits in Semiconductor Electronics," *Science*, March 18, 1977, pp. 1230–1235.
2. McKinsey analysis based on industry interviews.
3. Merrow, E.W. *et al.*, "Understanding Cost Growth and Performance: Shortfalls in Pioneer Process Plants," RAND Corporation Report R-2569 (1981).
4. "Who Is Top Management?," The Conference Board, Report no. 821 (1982).
5. Based on conversations with Bell Laboratories executives. For more on the development of the transistor, see Jeremy Bernstein, *Three Degrees Above Zero* (New York: Charles Scribner's Sons, 1984), especially p. 119.

IMPROVING THE RETURN ON RESEARCH AND DEVELOPMENT

A Stimulus for Thought and Action

Subcommittee on R&D Productivity
Research-on-Research Committee
Industrial Research Institute

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Monsanto Company
Northern Telecommunications Limited
Owens-Illinois, Inc.
Owens-Corning Fiberglas Corporation
Polysar Limited
RCA, Inc.
Rexnord, Inc.
Rohm and Haas Company
SPS Technologies, Inc.
Siecor, Inc.
Standard Oil of Ohio, Inc.
3M Corporation
Uniroyal, Inc.
United States Steel Corporation
Weyerhaeuser Corporation

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IMPROVING THE RETURN ON RESEARCH AND DEVELOPMENT

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PREFACE

This report is the principal product of two years of effort by the I.R.I. Research-on-Research Committee's Subcommittee on R&D Productivity. It incorporates the results of two surveys and a large number of other inputs and suggestions from the membership. The Subcommittee's efforts also produced a Special Interest Session at the 1983 I.R.I. Fall Meeting, and the Closing Session at the 1984 Annual Meeting focused on discussion of a draft of this report and of further suggestions for inclusion. Because of the magnitude of this effort and its success in providing results of lasting value, the I.R.I. Board felt it appropriate to publish this report and circulate it to the membership.

I wish to use this opportunity to express my personal appreciation to Jim McDonough, Lee Rivers, and Fred Kerst, Chairmen of the R-o-R Committee during this effort; Dick Foster of McKinsey & Company, Chairman of the R&D Productivity Subcommittee, as well as the other authors of the report and the other members of the Committee and Subcommittee.

Samuel W. Tinsley
President
Industrial Research Institute
1984-85

EXECUTIVE SUMMARY

The I.R.I. R&D Productivity Subcommittee was established to pursue an appropriate definition of R&D productivity, to develop a set of concepts to facilitate discussion and analysis of R&D productivity, and to provide a set of suggestions for the improvement of R&D productivity and the overall contribution of R&D to the corporation. This report is the final product of the Subcommittee's efforts.

The report is designed to provide a basis for several types of discussions: between R&D managers and their colleagues in R&D; between R&D managers and their counterparts in other parts of the corporation; and between R&D managers and their CEOs. The I.R.I. membership is diverse, and therefore the framework and suggestions discussed here must be adapted by each I.R.I. representative to the particular needs of his or her own organizational situation. The report is based on the opinions of experienced, practicing R&D directors, not on a third-party review of R&D performance or a comprehensive literature survey, although previous work on the subject has been built upon.

The report, and the work upon which it is based, is structured as follows. First, a conceptual framework and related set of terminology are described, in order to provide a structure for analyzing the contribution of R&D to meeting broader corporate objectives. Second, the prospects for improving that contribution are described, based in part on the survey results. Third, specific suggestions for improvement are offered, based on actions tried by I.R.I. representatives. Finally, some thoughts on how to utilize these results in a particular corporate environment are presented.

We assert that the primary variable of concern for measuring R&D performance is the ratio of R&D rewards to R&D costs. We call this ratio "R&D return," the broadest measure of performance in a corporate context. We define rewards as the net present value of future cash flow streams, and costs as the labor costs and other operating costs plus capital costs. This ratio is analogous to other economic return measures such as return on investment or return on equity. R&D return is the principal performance measure of interest to the CEO. The R&D return ratio, in turn, may be thought of as the product of other ratios: (1) the ratio of technical progress to R&D effort, which is our definition of R&D productivity; and (2) the ratio of profit to technical progress, which we propose calling R&D yield, a measure of the economic reward for bringing technological progress into the marketplace or the production process. By partitioning the R&D return ratio into these two subsidiary ratios, we can divide the problem conveniently into pieces that the R&D organization can directly influence (R&D productivity), and those that, by and large, it cannot itself control (R&D yield). Further, we can place R&D productivity in a broader context that shows its relationship to the principal objective of the corporation—profits.

The key to understanding R&D productivity is achieving the ability to analyze technological progress. Broadly speaking, technological progress is the difference between the current performance of the last generation product or process and the projected performance of the one now under development. The performance measure should meet at least four criteria: (1) it should relate to performance as the customer views it; (2) it should be a technical measure, not an economic one; (3) it should be a variable that the R&D department can influence; and (4) it should be measurable. Examples include energy consumption for light bulbs and tensile strength for fibers. In real life we often define multiple measures of technical performance, and often they can be combined into a single measure. R&D productivity can, in turn, be viewed as being comprised of "potential productivity," which is the maximum possible for a given technology, and "technology development efficiency," which is the efficiency with which this potential is attained by a particular company's organization.

R&D yield is a function of four variables: demand for the product; industry cost structure; competitive position of the company in its industry; and strategy of each competitor for the future. As with productivity, we can define "potential yield," which is the maximum possible yield for a given technology at a given level of development, and "operating efficiency," which is the degree to which the potential is attained by a company's organization.

This framework is relatively general in its applicability; it is based on a standard microeconomic analytic perspective. However, different parts of it will be of more or less relevance to particular I.R.I. representatives and their companies.

Use of this framework allows R&D practitioners to estimate the potential for improvement in the performance of industrial R&D organizations. The Subcommittee conducted two surveys to develop such estimates. The results indicate that the I.R.I. membership believes that there is a substantial potential for improvement in overall R&D return and in each of the four components of R&D return—potential productivity, technology development efficiency, potential yield, and operating efficiency. On average, survey respondents judge that there is for each component a potential for a 30 to 40 percent improvement in results. In aggregate, this leads to an overall average potential for improvement of over 100 percent in R&D return. These results held up consistently over two surveys taken by the Subcommittee. The surveys also showed a wide range in the views of participants for their own companies, from 10 to 60 percent potential for improvement. These potentials for improvement should not be taken as management targets; they represent the impossible ideal. For example, attainment of these levels depends on all other corporate departments—not just R&D—doing their jobs perfectly, or nearly so. The Subcommittee believes that a 20 percent improvement in any one of these four components would be a major achievement.

The Subcommittee also used the surveys to identify, from a total of 53 possibilities, the 13 activities that the I.R.I. membership believes have the greatest impact on R&D return in support of improvements in these four components.

These activities are as follows:

- Potential productivity:
    - Identifying technical possibilities
    - Identifying limits
    - Characterizing technology
- Technology development efficiency:
    - Professional personnel quality
    - Identifying projects
    - Project staffing
    - Project planning
    - Project termination
- Potential yield:
    - Demand/demand outlook
    - Strategies of all competitors
- Operating efficiency:
    - Identification of customer needs
    - Coupling to technical efforts—marketing
    - Coupling to technical efforts—manufacturing

A large number of operational suggestions—totaling 42—were contributed by I.R.I. members as ways of improving the performance of R&D organizations in support of these 13 return-improving activities. The 42 are summarized in Appendix B, while eight are presented in greater detail in the main body of the report. In total they offer a broad range of ideas from which R&D managers can draw.

Through the use of this framework and the suggestions for improvement, the Subcommittee hopes that I.R.I. members can boost the performance of their organizations. The framework can be used as a tool for gaining a common language for discussions within the corporation. An internal survey, similar to that used by the Subcommittee, could be performed to highlight the return components worthy of emphasis in the particular company. Finally, suggestions for improvement, using those made here as a starting point, could be solicited internally and thus used as a basis for action.

INTRODUCTION AND BACKGROUND

The effectiveness with which our nation's industrial research and development (R&D) laboratories pursue their work is a subject of great national importance and a major responsibility of the membership of the Industrial Research Institute (I.R.I.). In mid-1982, the I.R.I.'s Research-on-Research Committee (R-o-R) was commissioned by the membership to study the "productivity" of R&D; productivity is of course a concept widely used for analyzing the efficiency of economic or organizational performance. In response, the R&D Productivity Subcommittee was established to pursue the definition of an appropriate productivity measure. The Subcommittee was also charged with developing a set of concepts to facilitate discussion, and with providing a set of suggestions on how to improve R&D productivity and the overall contribution of R&D to a corporation. This report is the principal product of that Subcommittee.

Work of the Subcommittee

The Subcommittee proceeded as follows. First, it drafted, circulated, revised, debated, and adopted a set of concepts and definitions for viewing R&D productivity in a broader corporate context—"R&D return." This conceptual framework identifies four main components of R&D return; they are presented, defined, and discussed in later sections of this report.

Second, in August and September 1983, a preliminary survey based on this framework was circulated among Subcommittee and R-o-R Committee members, who estimated the potential for improvement in each of the four components of R&D return (including R&D productivity). Further, they assessed the relative importance of a comprehensive set of 53 supporting activities that might be undertaken to improve those returns. The results of this preliminary survey were extremely encouraging: the respondents collectively estimated a high potential for improvement in every aspect of R&D return. The R-o-R Committee therefore concluded that it should continue this effort. It decided to modify the survey, repeat it with a larger group of I.R.I. members, and explore the results in a discussion format with a broad segment of the I.R.I.

Third, at the November 1983 I.R.I. meeting in San Francisco, a Special Interest Session (SIS) was held to present and discuss the results of the preliminary survey. This wide-ranging and thought-provoking session actively addressed several aspects of the framework and survey results. Attendees at the SIS volunteered to participate in a second survey.

Fourth, the second survey was developed and circulated to Subcommittee and R-o-R Committee members and SIS attendees in December 1983. It consisted of: (1) an abbreviated but closer look at estimates of potential for improvement in R&D return; (2) a reconsideration of the ranking of the activities which could lead to improved returns; and (3) a request to identify specific actions that had been used by I.R.I. representatives at their own companies to improve R&D return.

The results were received and compiled in January and February 1984. The survey was a substantial success. It had a high return rate. More significantly, it refined and largely reconfirmed the estimates which had been made in the initial survey of the potential for improvement, and it resulted in some adjustments in the rankings of the activities which could lead to increased returns. It also produced a range of specific actions which I.R.I. members could recommend to each other for use at their companies for improving R&D return. The results of this survey are presented and interpreted in this report.

Fifth, the Subcommittee prepared a draft of this report in April 1984 and circulated it to the I.R.I. membership before the May 1984 I.R.I. Spring Meeting. The final morning of the meeting focused on discussion of the report and of more suggested actions that might improve R&D productivity.

Finally, based on the discussion at the meeting and the follow-up, this final report was prepared.

Several other activities of the R-o-R Committee are under way which address aspects of R&D productivity. For example, there is an important activity of the R-o-R Subcommittee on Computer-Aided R&D. Similarly, there is a Subcommittee on Corporate Relations which has drafted a preliminary paper entitled "Improving Relations Between R&D and Other Corporate Functions." The R-o-R Committee has of course also worked closely with the NSF-University of Cincinnati study of industrial innovation, which sheds much light on many topics addressed here. For obvious reasons, the R&D Productivity Subcommittee has not addressed many of these specific topics in detail and this report is not intended as a substitute for these more focused but parallel efforts.

Purposes of This Report

The purposes of this report, as of the study on which it is based, are to present a framework for thinking about the performance of R&D organizations, the potential for improving that performance, and the kinds of activities most likely to realize that potential. It is designed to provide a basis for several types of discussion: between R&D managers and their colleagues in R&D; between R&D managers and their counterparts in other parts of the corporation (general managers of operating divisions or managers of other functional departments such as marketing or manufacturing); and between R&D managers and their CEOs. It is intended to be provocative—especially since many of the areas identified by I.R.I. members as central to major improvements involve relationships between R&D and its sister functions. The R&D organization cannot by itself realize the full potential for improvement.

Some Caveats

The I.R.I. membership is diverse, and each of its member companies is unique. The framework and suggestions discussed here must be adapted by each I.R.I. representative to the particular needs of his or her own organizational situation. There are no simple and general answers on how to analyze or improve R&D performance that apply to all R&D organizations, but there are principles which can be applied. This report is therefore intended to help I.R.I. representatives evaluate their own situations, using some simple principles, and develop ideas for improving the performance of R&D.

The report is based on the opinions of experienced, practicing R&D directors, not on a third-party review of R&D performance. Nor does it derive from a comprehensive literature survey, although the authors are aware of other work on the subject, e.g., the numerous articles on related subjects in RESEARCH MANAGEMENT, and have tried to build on prior ideas where possible.

CONCEPTUAL AND SEMANTIC FRAMEWORK

The search for methods of characterizing, analyzing, and even describing the performance of an R&D organization has long been—and will undoubtedly remain—the subject of much discussion in the R&D community. R&D productivity, in particular, is a complex subject. A precondition for addressing it in a coherent and consistent manner is a set of definitions for a group of measures which together provide a reasonably complete picture of R&D performance. The Subcommittee has developed such a set.

For a single product, as Exhibit 1 indicates, two factors determine "R&D return," the broadest measure of R&D performance in a corporate context. These are "R&D productivity" and "R&D yield," each of which is driven by different forces. From this set of terms flows the basic premise on which our measures rest.

Basic Premise

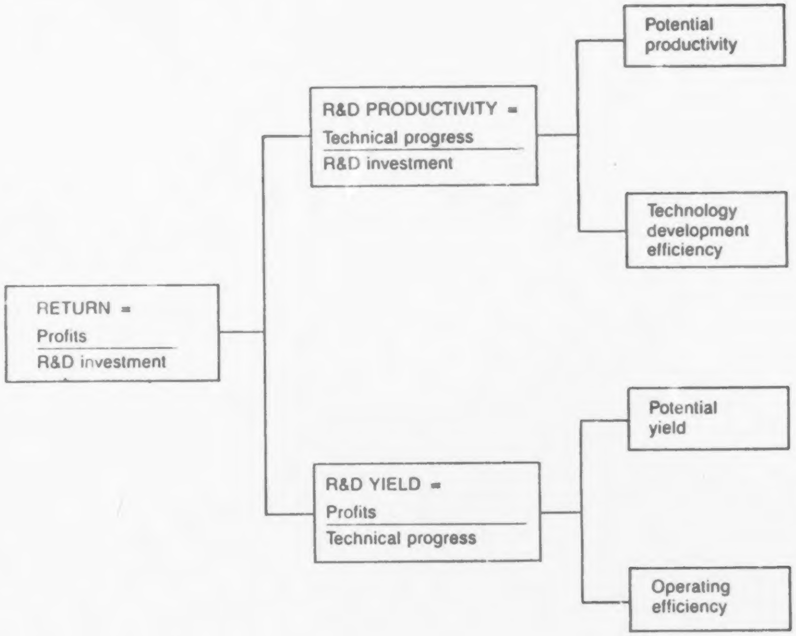
We assert that the primary variable of concern is the ratio of R&D rewards to R&D cost. We define rewards as the net present value of future cash flow streams and cost as labor cost and other operating costs plus capital costs (we will also refer to this total cost as R&D "effort" or R&D "investment"). For convenience, we propose calling this ratio "R&D return," since it is analogous to other economic return measures such as return on investment or return on equity. R&D return is the principal R&D performance measure of interest to the CEO.

The R&D return ratio, in turn, may be thought of as the product of two other ratios:

1. The ratio of technical progress to R&D effort, which is our definition of R&D productivity (technical progress is measured by performance variables that determine the economic value of the technology to the customer); and

Exhibit 1

R&D RETURN, PRODUCTIVITY, AND YIELD FRAMEWORK


RETURN = Profits / R&D investment
R&D PRODUCTIVITY = Technical progress / R&D investment
R&D YIELD = Profits / Technical progress
Potential productivity
Technology development efficiency
Potential yield
Operating efficiency

INTENTIONAL
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2. The ratio of profit to technical progress, which we propose calling R&D yield, since it is a measure of the economic reward for bringing technological progress into the marketplace or the production process.

By partitioning the R&D return ratio into these two subsidiary ratios, we can divide the problem conveniently into pieces that the R&D organization can directly influence (R&D productivity) and those that, by and large, it cannot itself control (R&D yield). Further, we can place R&D productivity in a broader context that shows its relationship to the principal objective of the corporation—profits. Thus productivity, not yield, becomes the component that R&D management should principally be accountable for and measured by. Other functional managers, of course, must be accountable for R&D yield if the overall value of the R&D effort is to be maximized; these may include planning, marketing, manufacturing, or product management, depending on the company.

R&D Productivity

The key to understanding R&D productivity is achieving the ability to analyze technological progress. We must also be able to estimate the R&D effort responsible for any given technological advance.

Technological progress. Broadly speaking, technological progress is the difference between the current performance of the last generation product or process that the company introduced and the projected performance of the one now under development or about to be introduced. Strictly speaking, though, there is no requirement that introduction to commercial use or any other set point in the life cycle be the fixed time at which to measure progress, as long as that time is consistent for both the old and new product or process.

The product (or process) performance measure should meet at least four criteria:

- *It should relate to performance as the customer views it*—i.e., to the benefits the customer perceives that he or she can receive by using the new product or process in place of the one now in use.
- *It should be a technical measure, not an economic one* (e.g., tensile strength or yield, not profit or price) so that we can separate technological from economic considerations. Economic measures may be used, of course, but only if they represent essentially physical attributes.
- *The performance measure should be a variable that the R&D department can influence*, since that is ultimately what the department will be expected to do. Thus, the measure becomes a target that R&D can use in setting its objectives and that management can use in judging R&D performance.

- *The variable should be measurable*—for example, energy consumption for light bulbs, tensile strength for fibers, barrier properties for plastic packaging films, instructions per second for computers, corrosion resistance for metals, yield for chemical and electronic processes, and selectivity for chemical reactions. In real life, we often apply multiple measures of technical performance. In tire cords we look for tenacity, abrasion resistance, and corrosion resistance; but since the customer is usually aware of the trade-offs among these variables, we can often combine them into a single measure.

In summary, relevant performance measures for R&D products must recognize the customer's interests, be technological, relate to something that a scientist or engineer can do, and be measurable.

The relevant performance measures may change over time due to changing needs or changing technology. For example, in the 1960s octane was a key measure for gasoline, but in the 1970s emission of pollutants approached it in importance. Similarly, before CAT scanners appeared, radiation dose level for a given image quality was perhaps the most important measure of X-ray machine performance; after CAT scanners, new parameters such as speed of scan became important. In these cases, new productivity measures have to be adopted and old ones forgotten. (There is some evidence, however, that it is not unusual for performance measures to remain stable over a decade or more.)

R&D effort. Usually the effort that went into achieving a given technological advance represents only part of the total R&D effort devoted to the product line. The balance comprises plant technical service, customer service, applications engineering, and other efforts not dedicated to advancing the technology *per se*. To estimate the "advance" portion of the total, which is the most relevant portion for this analysis, we use a measure that includes both a labor component and a capital component (expressed in constant dollars).

Productivity uses and misuses. Given a definition of R&D productivity, we need to examine what it can tell us. We also need to show how it can be mis-estimated. When measured by product or process performance, R&D productivity changes from a "gut" feel or matter of belief into something we can analyze. Briefly, it appears to be a function of two factors that are controllable primarily by the R&D department. The first is the choice of basic technology, which sets limits on the ultimate performance achievable, e.g., rayon tire cords could never be as strong as polyester tire cords. Thus it determines "*potential productivity*." The second is the efficiency with which this technical limit is pursued. This "*technology development efficiency*" is affected by at least four variables: (1) the base of knowledge about the technology—specifically, how the product or process is put together (its "structure"), what input variables can be manipulated (e.g., degree of cross-linking in a tire fiber), and how changes in input variables affect changes in output variables (e.g., tire lifetime); (2) the timing of the effort—crash projects cost extra money, often a lot of it; (3) operations efficiency or support, which includes computer and instrument support, personal skills mix and quality, and organization and management methods; and (4) management systems for initiating, planning, staffing, and terminating projects.

In short, we can analyze R&D productivity, as defined, for any given product line at any given point in time. We can also analyze changes over time. These changes are largely the consequence of decisions taken within the R&D department.

In attempting to apply the methodology described here for analyzing R&D productivity, some managers have experimented with surrogate measures for both R&D effort. Surrogates are generally unsatisfactory, however. To illustrate:

- Surrogate measures for R&D output might include, for example, the number of new products introduced, patents awarded, and so on. At first blush these measures seem like reasonable yardsticks for output. On closer examination, however, they do not hold up because they implicitly assume that the relationship between the surrogate (whatever it is) and technical progress (in terms of value to the customer) is linear. In general, it is not. As products mature, to cite a familiar instance, the differences between each succeeding generation of products diminish, but the rate of new product introduction may not change with respect to time because competitors still fight for an ever-shrinking competitive advantage. Hence, use of the surrogate measure could well mask the phenomenon of declining (or increasing) returns.
- The most popular surrogate measure for effort is time, but time will work as a surrogate only if it is linearly proportional to effort, which is seldom the case. Because the level of effort often increases with time, this surrogate will often distort, and distort significantly, the final results. For example, if we plot component density versus time in microelectronics, we see "exploding" progress; if we plot the same data versus effort, we see diminishing returns.

R&D Yield

As discussed above, R&D yield is the ratio of the profits obtained from a technical advance to the increment of technical progress represented by that advance. Unlike productivity, R&D yield is not principally controlled by technology and the R&D department but, rather, by the laws of supply and demand and the effectiveness of the corporation in responding to these laws. Consequently, it is a function of four variables:

- *The demand* for the product and, by extension, the elasticity and cross-elasticities of that demand.

- *The industry cost structure*—specifically, the differences between the high- and low-cost producers, the differences between the economic value the product has for individual customers, the reinvestment hurdle rate, and the variable costs.
- *The competitive position* of the company in its industry. Given the same potential for technological advance, a new low-cost competitor has an advantage over an old high-cost competitor.
- *The strategy* of each competitor for the future. If all competitors add capacity faster than demand is growing, all competitors lose.

Clearly, the R&D department cannot directly have a dominant influence on any of these variables. They are the primary concern of other corporate managers. Often, even the company acting alone cannot have any control over them. Rather it is the collective actions of all competitors, not the wishes of any single competitor, and the relative power of suppliers and buyers that influence the profits in an industry.

As with productivity, we can define a "*potential yield*," which is the maximum yield possible for a given technology at a given level of development, and an "*operating efficiency*," which is the degree to which the potential is attained by a company's organization.

Without doubt, R&D yield varies greatly among companies, perhaps even more than does R&D productivity, but little real evidence has been gathered to date. What there is, is anecdotal and must be put to rigorous explication and testing. For the time being, therefore, we cannot assume a priori that R&D productivity or yield is the most important component of R&D return. Nor can we assume a priori that the rate of technical progress is more or less important than the work of other functions in maximizing R&D return. We have to approach the problem on a case-by-case basis. Analyzing R&D productivity over time may allow R&D management to detect long-term or short-term improvements or deterioration earlier than would have been possible in the absence of analysis. Equally important, it gives the manager a basis for analyzing the causes of changes over time and thus leads to the identification of potential solutions—e.g., switching technology or increasing computer support (suggestions for such action steps will be discussed later in this report).

Exhibits 1 and 2, respectively, illustrate the relationships between and provide definitions of these concepts which the Subcommittee has used in its surveys and analysis.

Insights Possible From Measures

Using the formula R&D return equals R&D productivity times R&D yield, we gain some potentially useful insights that a company's Vice President of R&D and CEO can share, for example:

Exhibit 2

DEFINITIONS OF KEY CONCEPTS IN R&D RETURN FRAMEWORK

R&D Return. The amount of profit, or net present value, expected from an investment in R&D. This is assumed to be the objective, at least implicitly, for which most CEOs strive. It is the consequence of a corporation's success or failure in achieving a sustainable competitive edge. The units of return are measured in $ net present value per $ investment.

R&D Productivity. The improvement in key performance parameters (product or process) due to investment in R&D. Productivity is measured by units of performance improvement per $ R&D investment.

R&D Yield. The profit made from improvement in technical performance—that is, the economic reward for the investments made. The unit of measurement is $ net present value per unit of performance improvement.

Potential Productivity. The maximum possible productivity improvement as set by the limits of the technology and the best technology development organizations.

Technology Development Efficiency. The efficiency of the R&D organization compared to the maximum possible as conceptualized in "Potential Productivity." Said another way, this is the relationship of the development effectiveness of a given competitor to the best possible competitor.

Potential Yield. This is the maximum economic return possible given the structure of the market. To achieve these returns would imply perfect operational effectiveness—no mistakes in plant start-up, communication, or planning.

Operating Efficiency. The efficiency of "commercialization effort." This term relates the actual costs and rewards of commercialization to the costs and rewards of a "no error" introduction.

- R&D productivity and R&D yield may, in some cases, be completely independent. R&D productivity may, for example, be positive when R&D yield is negative (as when technological progress is possible, but the industry has and will continue to have substantial overcapacity). Since the negative yield implies a negative return, the R&D organization should not undertake more technological work—no matter how much it intuitively favors it or how high its productivity—unless management has reason to believe that R&D yield will become positive in the future. By contrast, R&D yield may be positive and R&D productivity near zero. Although marketing or management may want to sponsor R&D efforts because of the perceived high payoffs, it should not do so unless R&D productivity can be improved—as, for example, by changing technologies (to get around limits of the present technology or to increase overall efficiency).
- A positive R&D productivity, taken without regard to R&D yield, may lead a company to seek to "really capitalize" on its strong technological lead. In an excess of enthusiasm, combined with what may turn out to have been unfortunate planning, this strategy may lead the company to overexpand, triggering a fall in market prices and thus a negative R&D yield. Alternatively, some companies may simply have a technology delivery problem, i.e., a limited capability for bringing desirable technical advances successfully into the marketplace—which might also result in a negative yield.
- Both R&D productivity and R&D yield may be positive, but their product—R&D return—does not exceed the financial returns required based on the corporation's cost of capital. In this case, further investment is not warranted because the other options for investment available either to the management or to the shareholders are more attractive.

Applicability

Within the limitations described above, this framework is fairly general in its applicability; it is based on a standard microeconomic analytic perspective. However, different parts of it will be of more or less relevance to particular I.R.I. representatives and their companies. The I.R.I. membership is diverse, and the applicability of this framework and its particular pieces must be tailored to each individual organization. Corporate research laboratory heads will focus primarily on research; product-line R&D managers, on development. These differences in focus drive several differences in the framework's application. Further, each I.R.I. representative and his or her organization have strengths and weaknesses which may be quite different.

For example, an I.R.I. representative who is the head of a central corporate research laboratory in a company with many operating units will probably be most interested in productivity items, such as identifying technical possibilities not being currently examined by the company, or in determining technical limits which may be far out on the scientific horizon. Such an R&D director will leave concerns for the more immediate product/market relationship to product development managers within the operating divisions. An I.R.I. representative who is the R&D manager in a company with limited product lines will be closer to product development in his or her own work and will probably be more concerned with yield issues such as market relationships, competitor activities, and so on.

Similarly, corporate research laboratory directors, in terms of their internal relationships, will be dealing with business-unit general managers and attempting to transfer their technology to these general managers and their product development managers. R&D managers in companies with limited product lines will deal not only with their general manager but also with the heads of marketing and manufacturing operations. A further distinction must be made for R&D managers whose work is oriented primarily toward improvement of technology to be used in process applications (where the customer would be internal) and those whose work is devoted to product technology (where the customer would be external).

Everything we have proposed so far has implicitly assumed the context of a single product line, but most corporations, of course, have more than one product line. These multiple lines raise the issue of comparability of R&D productivity between product lines. Clearly, if the measures of performance are different for different product lines, which we presume they usually are, they will produce different productivity results. As discussed above, the Subcommittee has used these concepts qualitatively, for which purpose they are sufficiently precise for application to multiple product lines.

As a final note, we must emphasize that this framework is not presented here as a guide to the quantitative analysis of R&D returns. Quantitative analysis, based on this or other models, has often been carried out quite successfully for individual product or process technologies in specific markets or for particular companies. Such an analysis would not have been possible in a manner consistent with the scope of this effort. Therefore, the R&D Productivity Subcommittee has not used the framework for quantitative analysis but, rather, as a conceptual structure to aid discussion and analysis of R&D performance within the broader context of the business environment.

POTENTIAL FOR IMPROVEMENT IN R&D RETURN

Use of this framework allows R&D practitioners to estimate the potential for improvement in the performance of industrial R&D organizations. The Subcommittee conducted two surveys to develop such estimates.

The I.R.I. membership believes that there is substantial potential for improvement in R&D return and in each of its four components: potential productivity; technology development efficiency; potential yield; and operating efficiency. On average, survey respondents judge that there is for each component a potential for a 30 to 40 percent improvement in results. In aggregate, this leads to an average overall potential for improvement of more than 100 percent in R&D return. These results held up consistently over the two surveys undertaken by the Subcommittee.

The surveys also showed a wide range in the views of the respondents about their own companies (respondents indicated between a 10 and 60 percent potential for improvement) and no significant distinction could be found between the average potential of the four components of R&D return. Details concerning the survey results are available from the Subcommittee.

In an absolute sense, the opportunity is, according to the I.R.I. survey respondents, immense. The opportunity could be viewed as possible improvements in the impact of the organization, or a combination of the two. On a relative basis, one possible comparison might be to speculate on what a comparable survey might reveal for some other corporate functions, e.g., marketing, manufacturing, or transportation. While the Subcommittee is not aware of any comparable data, one might ask whether the effectiveness of these functions might have the same potential for improvement.

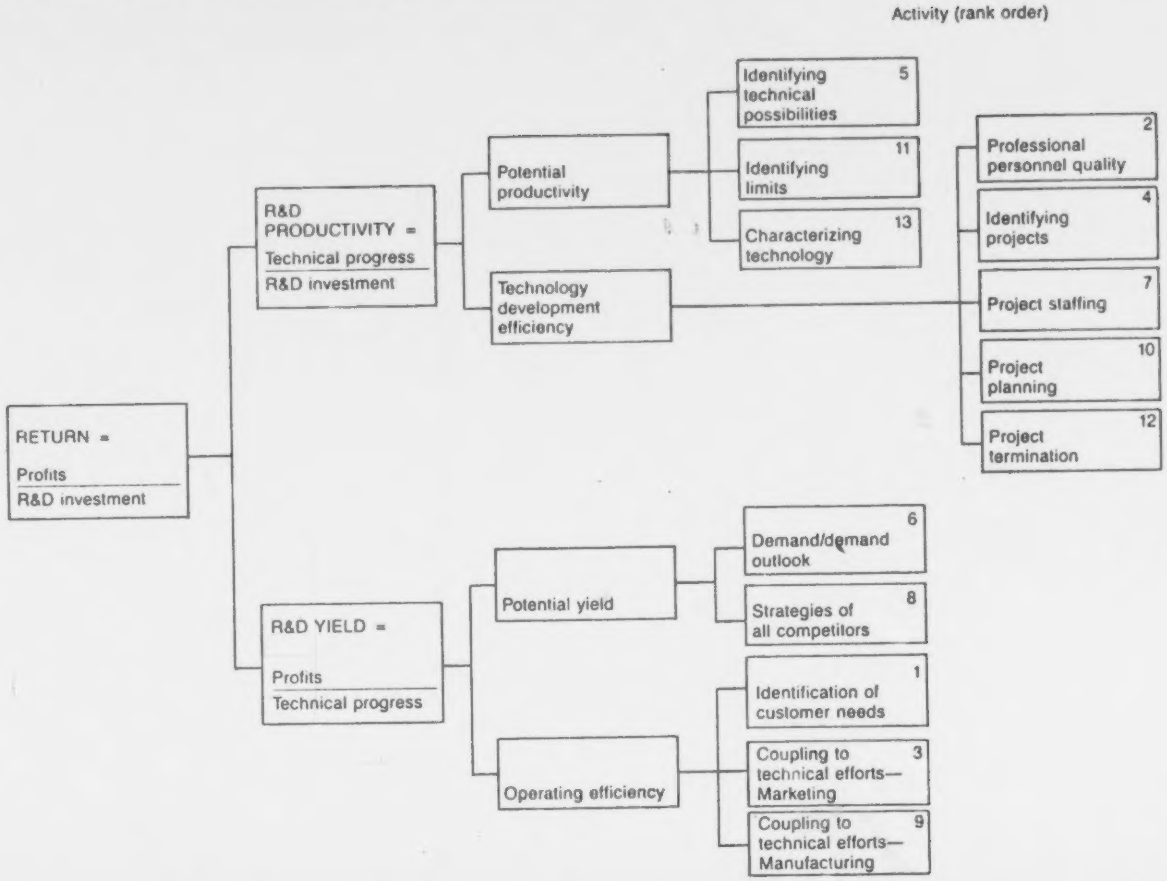
These potentials for improvement should not be taken as management targets; they represent the impossible ideal. Reaching the 30 to 40 percent level depends, for example, on all corporate departments—not just R&D—doing their jobs perfectly, or nearly so. Any R&D manager or CEO should set a more realistic target for what can actually be attained. The Subcommittee believes that a 20 percent improvement in any one of these four components would be a major achievement.

These results reflect the tremendous diversity among I.R.I. member companies with respect to the strengths, weaknesses, and potential for improvement of their R&D departments and operations. There is a broad range of performance among the current I.R.I. membership. Even though some companies are performing closer to their potential than others, there is broad agreement among them on the general range of potential for improvement (more than half the results fall in the 20 to 50 percent range).

Exhibit 3 identifies the 13 activities that the I.R.I. membership believed to have the greatest impact on R&D return and relates them back to the general framework. The rank order among them indicated by the survey results is as shown. These thirteen activities were selected from a much larger group of the 53 presented in the survey (the full list appears in Exhibits 1 and 2 of Appendix A, along with definitions of the top 13). The rank order of the 13, refined on the basis of both surveys, will not surprise practitioners or knowledgeable observers, although there has been extensive discussion with the Subcommittee, at the SIS in November 1983 and elsewhere of some of the items which did not make the top 13. Such R&D aids as computers and such project management issues as cost estimating and process engineering are obviously important, but according to the I.R.I. survey respondents, not as important on average as the 13 indicated.

Exhibit 3

R&D RETURN FRAMEWORK—HIGH RETURN ACTIVITIES


Activity (rank order)
RETURN =
Profits
R&D investment
R&D PRODUCTIVITY =
Technical progress
R&D investment
R&D YIELD =
Profits
Technical progress
Potential productivity
Technology development efficiency
Potential yield
Operating efficiency
Identifying technical possibilities 5
Identifying limits 11
Characterizing technology 13
Professional personnel quality 2
Identifying projects 4
Project staffing 7
Project planning 10
Project termination 12
Demand/demand outlook 6
Strategies of all competitors 8
Identification of customer needs 1
Coupling to technical efforts—Marketing 3
Coupling to technical efforts—Manufacturing 9

In response to the request from the Subcommittee, I.R.I. representatives offered numerous suggestions for actions which they had taken in their own organizations to improve R&D productivity or R&D yield in support of these 13 activities. A summary of the most relevant and substansive of the suggestions, along with a number from the literature, is presented in Appendix B, and several of the most detailed are quoted extensively below.

SOURCES OF IMPROVEMENT IN R&D RETURN

The substantial perceived potential for improvement leads naturally to an inquiry into the possible sources of this improvement. The conceptual framework and survey results enable us to understand these sources. They also allow us to lay out a set of suggestions for realizing some of this potential in practice.

Areas of Improvement

We can improve *potential productivity* by doing a better job of identifying new technical possibilities, of understanding and characterizing the technologies we are working with, and of identifying limits to those technologies. This means, for example, taking a broad and holistic approach when viewing the alternatives to the current technology and not an incremental one. It may mean appointing a gatekeeper who would be responsible for scanning the horizon and bringing new ideas into the organization, as suggested by Allied Corporation and discussed below. It may mean setting about in a systematic way to explore the limits of current technology and of competing technologies, whether or not they are being investigated by competitors.

We can improve *technology development efficiency* by ensuring that we have the highest quality professional staff, by taking special care in identifying the particular projects to be pursued, and in staffing, planning, and terminating those projects. This may mean, for example, as suggested by GTE, choosing project leaders who are dedicated and effective managers but who may not necessarily be the most intelligent or knowledgeable scientists or engineers. It may mean improving the quality of the new recruits by enhancing the laboratory's reputation through increased publication and small grants to universities, as suggested by American Cyanamid.

We can improve *potential yield* by doing a better job of understanding how technologies can best fit into the marketplace—specifically, by paying more attention to the demand for new products derived both from research and from knowledge of what competitors are doing. This may mean more involvement of the marketing department up front so that greater marketplace success is achieved for the technologies chosen for market introduction. It may mean a close examination of how new products actually perform in the marketplace, as compared to expectations, so that the structure of demand can be better understood and forecasts made more realistic. It may mean a focused effort within the R&D organization to analyze the gaps between the technical performance of competitors' products as compared to our own, as is done at Mead.

Finally, we can improve *operating efficiency* by identifying more clearly the needs of customers and working more effectively with the manufacturing and marketing organizations to meet those needs. This may mean, for example, utilizing a formal document with sign-offs by manufacturing and marketing, such as is used at Abex and GTE. It may mean putting R&D engineers or scientists to work on identifying new uses for current technologies; Polysar's recent experience is presented below. It may mean joint staffing of projects with manufacturing engineers as well as researchers, as is done by Eastman Kodak. It certainly means developing better communication between R&D managers and corporate planners. Poor communications on this front have been well documented in surveys taken at I.R.I. Senior Management Workshops, where consistently different views have been discovered between corporate planners and R&D managers from the same companies.

As a stimulus to discussion and experimentation by I.R.I. representatives, the Subcommittee felt it would be useful to present verbatim some of the most articulate and potentially productive of the suggestions offered. Each relates to one or more of the four main components of R&D return and one or more of the most important return-improving activities.

Idea No. 1: Appoint a Technology Gatekeeper.

Source: Allied Corporation
Principal return component: potential productivity
Principal activity: identifying technical possibilities

"Keeping up with the state-of-the-art in relevant technologies is a difficult task for any company. Many rely on the individual researcher to 'know' his field. Certainly that has to be your first line of defense.

"If you have an outstanding scientist who exhibits 'generalist' tendencies you may have another alternative that can effectively augment that first line of defense. Appoint your outstanding scientist as your 'Chief Scientist' and give him or her a broad charter to assist your technical program managers in solving difficult technical problems and to act as a scientific or technologies 'gatekeeper.'

"Your Chief Scientist, to be truly effective in his or her role as gatekeeper, has to have a broad technical base, strong scientific credentials, and the desire to serve others by applying his or her curiosity to their problems. He or she needs to be a good communicator, an avid reader, and a person with broad personal contacts in industry, universities, and professional associations.

"Clearly this is not a job for everyone. In fact you may not have a suitable candidate in your organization, but when you do, using him or her as your Chief Scientist and Technology Gatekeeper can prove to be a major productivity improvement step."

Idea No. 2: Initiate a Formal Idea-Generation and Screening Program.

Source: Weyerhaeuser Corporation
Principal return component: potential productivity
Principal activity: identifying technical possibilities

"Some years ago we established within the company's R&D organization a program for generating and screening new business (or new product) ideas. The actions taken relate primarily to 'identifying technical possibilities' and lead to the improved identification of products. They also touch a number of the other return-improving activities such as project termination, identifying customer needs, and coupling technical efforts to marketing and manufacturing. The intended result of the program was to lead the company into profitable new business or new product areas.

"The program consisted of several parts: structured idea-generating techniques; a future-oriented set of descriptive scenarios (out 25 to 30 years) which was used as a 'mind-expanding' tool to stimulate ideas; a set of idea-screening criteria; and a programmed series of steps for evaluating and accepting or rejecting ideas as they progressed through research and development. The whole system was built around social, political, and economic dimensions, as well as technical. Business and operating people were involved at all steps along the way. The screening and evaluation criteria included not only technical, economic, and market factors, but some that were more intangible—such as how well the idea fits with past company behavior and an assessment of the probability that top management support can be obtained.

"Many hundreds of ideas have worked their way through the program with many of them being rejected, of course. Some ideas emerged as successful new businesses for the company which are now contributing significantly to profits."

Idea No. 3: Use Technology Limits Analysis to Help Structure R&D Program.

Source: Owens-Corning Fiberglas Corporation
Principal return component: potential productivity
Principal activity: identifying limits

"Several years ago, we began to be concerned that one of our products might be mature since technical process seemed very slow; consideration was being given to cutting back on R&D funding. Further, two competitive products had picked up market share against ours—one in specialized applications where customers were willing to pay more for a higher cost product, the other more broadly due to its aesthetic advantages—and we were uncertain as to how far they might go. We decided to undertake an investigation of the technical limits of both our product and the competition's.

"We started by trying to get a precise view of which technical factors of our product were most important to its users. We then sought to determine both the theoretical and practical limits to these factors. For several of the factors, we found we were very knowledgeable and were able, with some effort, to determine the limits. In fact we discovered substantial remaining technical potential and developed several ideas for attaining some of them without serious increases in cost. For one of the factors, we found that having relied in the past on an empirical understanding of the inherent properties of the material, we could not really determine the limiting mechanisms, we therefore initiated a research program directed toward this end. Research was also undertaken to determine the technical limits of the competitive products. For the broad-based competitive product we found that the technical factors were the same as for our product, but the limits to these factors were different; this gave us a set of research targets for our product. For the specialized competitive product the technical factors themselves were different.

"We found that a systematic identification of the technical performance factors determining the value of our product and a clear view of the mechanisms limiting those factors were very helpul in directing our research efforts. We accelerated our research, to reduce the gap between the performance of our product and its limits and those of the competitive products."

Idea No. 4: Develop a Technology Catalog.

Source: Rohm and Haas Company
Principal return component: technology development efficiency
Principal activity: identifying projects

"We have developed a 'Technology Catalog' for use as a planning tool to assist us in visualizing how technology is or can be used to generate and sustain competitive advantage for a specific business. To do this, we have identified the technologies which are important to the development and manufacture of each of our major product lines. Once listed, we have evaluated these technologies four ways:

- Importance of the technology: Does the technology differentiate competitors and give competitive advantage?
- Emphasis of the technology: Is innovation in the technology directed more toward manufacturing-cost reduction or product performance?
- Life-cycle position of the technology: How mature is the technology? How much more difficult will it be in the future to develop or maintain differentiation and competitive advantage through this technology?
- Our competitive position within the technology: How do we compare to the competitors in practicing this technology? Who is most likely to gain or maintain advantage?

"By looking at each technology supporting a product line according to these four criteria (both today and as expected in the future), we have found it possible to obtain valuable insight into the probability of commercial success for alternate technological strategies. To do this, we consider and discuss such issues as:

- What technologies are likely to differentiate competitors and lead to competitive advantage in the future?
- How likely or unlikely is significant innovation in these technologies?
- Are we likely to be the beneficiaries of innovation or are our competitors?
- Are we placing our resources into technologies which promise to yield the greatest impact?
- Can nontechnological strategies overcome technological deficiencies?

"Our major businesses were each found to have from 1 to 13 important technologies. Yet the great majority had only 2 to 5 'key' technologies (ones differentiating competitors and giving a competitive advantage) and 1 to 2 'pacing' technologies (ones which do not give competitive advantage at present but probably will in the future). No attempt was made to generate a numerical model to equate status of technology with research or business strategy. Instead, generalizations were developed as guidelines for developing strategic options. These include:

- As technologies mature, it becomes more difficult and more expensive to innovate in such a way as to gain advantage for any sustained period of time. As a consequence, resources invested in maturing technologies may not be rewarded by effective improvement in competitive positioning.
- Complete withdrawal of technical resources from a mature base technology can often lead to loss of competitive advantage. Product quality may deteriorate; minor innovations by competitors may be difficult to copy.
- A weak position in pacing technologies can lead to significant loss of competitive advantage if that technology suddenly becomes key. A competitor with a major market position adopts a high-risk strategy by remaining weak in pacing technologies. Conversely, a competitor weak in the key technologies can develop significant advantage by investing in a pacing technology and driving it to become key.

Many other generalizations become apparent when various combinations of importance, emphasis, life cycle, and competitive position are considered.

"The Technology Catalog has provided a format and vocabulary which has clarified communication between research and business management. It has allowed focus on technology whose risk/reward balance is most favorable."

Idea No. 5: Systematically Estimate R&D Return for New Opportunities.

Source: Abex Corporation
Principal return component: technology development efficiency
Principal activity: identifying projects

"It was generally agreed that too few new, commercially viable products were emerging from our Research Center during the mid-1970s. We therefore set up a program with the intended result of getting more successfully implemented new products for our investment in R&D. A constraint imposed was that the level of R&D expenditures, as a percent of sales, could not be increased. The program included the careful analysis of our R&D projects. It also established a mechanism for all key players to have input into the definition and selection of R&D projects, as well as to participate in the management of projects and monitor their progress.

"We began by recognizing that not all activities conducted at the Research Center actually constituted research or development. Projects were classified as research, development, or one of several other categories (applications engineering, cost reduction, quality control, customer service, etc). This allowed further determination as to whether a project belonged in the 'base case' or represented an 'opportunity module.' The former were defined as those projects associated with ongoing businesses or existing products, while the latter represented projects aimed at new products or technologies. True research and development projects thus tended to fall in the opportunity module category. Base case projects were assessed versus the continuing level of earnings, while opportunity modules were viewed in terms of the incremental earnings to be expected from successful introduction. We did not try to measure R&D productivity on base case projects.

"For an opportunity module, all projected costs that could be identified over a 5-year period—including capital—were divided into the expected margin dollars to be generated by the product. This margin number was typically estimated for a 10-year period, in recognition of the life cycle experience for profitability upon new product introduction. If a project came out with a rating of less than 1, it was discarded, or its proponents had to be able to demonstrate why it should be funded in spite of apparently poor return. When this analysis was used in R&D planning cycle for the first time, it resulted in the cutting out of over 60 percent of the proposed projects.

"The remaining projects were ranked in descending order, to help assure that resource allocation was related to the potential impact of the project. While this process was a part of the planning and budgeting process, means were also established for an iterative sign-off process before actual R&D work began. Under this system, a document detailing the project's objectives, timing, financing, and other constraints was prepared and approved by the appropriate division president, sales, marketing, manufacturing, and engineering executives, as well as the research director. A regularly scheduled series of review meetings among the same group assured that projects stayed on track and were terminated or altered as appropriate.

"Such a mechanism tends to make the R&D department a partner, rather than an adversary, of operating divisions. It opens R&D planning to a broad range of managers from other groups within the company. While this may result in some loss of anatomy for R&D, it also promotes a commitment from non-R&D managers to support research."

Idea No. 6: Establish Specially Selected R&D Task Teams.

Source: Manville Corporation
Principal return component: technology development efficiency
Principal activity: project staffing

"At Manville we set up task teams focusing on what we call the 'four P's'—People, Profile, Problem, and Process.

"*People.* We have made a conscious decision to make extra efforts to seek out, for use in a number of R&D efforts, people from various areas—research, marketing, engineering, finance, etc.—who are identified by peers, superiors or even subordinates as creative as well as knowledgeable individuals. We have also worked at developing a climate that permits the freedom to innovate and encourages prudent risk-taking. The selection of a particular type of individual who will respond favorably to this type of climate is made through the use of what we designate as our 'profile.'

"*Profile.* Our profile of the creative and contributory individual for research projects is still evolving. However, there are some traits that we have identified as being important.

- Creativity. The person must have an ability to look beyond the narrow confines of his or her assignment. They are 'blue sky' thinkers.
- Compatibility. We work hard at being sure, through a question and discussion process, that those people assigned to a project will be able to work well together.
- Self-assurance. People we select all have a reasonable degree of self-confidence. Consequently there are no barriers that are set up due to ego considerations. Our productive individuals also are: uninhibited, driven by a sense of urgency, unencumbered by fear of failure, spontaneous, able to use an intuitive approach to things, and able to work in an unstructured environment. When we have, through use of this profile, identified individuals for our projects, this leads us to the final two P's, 'problem' and 'process.'

"*Problem.* We have utilized our approach to address 'problems' of new product development, underutilization of physical assets, quality concerns, and a number of other issues.

"*Process.* In summary form, the key components of the process, after identification of the people and the problem, are: no formal charter or procedures, limited duration of assignment (usually 30 to 60 days), a predetermined date midway through the assignment when reports of results are presented, no designated leader, no ideas to be rejected, and a goal-setting process. In the work itself, we have a planned approach in which specific literature is introduced which allows the people to focus on both the problem at hand and the creative mentality. Fundamental to the process is the use of the task team approach usually involving five to seven people.

"Our results have been extremely gratifying, not only in terms of the actual problem-solving, but also because of the serendipity benefits that have come from the new ideas."

Idea No. 7: Appoint an Individual Responsible for Identifying New Uses of Current Products.

Source: Polysar Limited
Principal return component: operating efficiency
Principal activity: identifying customer needs

"In one of our businesses we created a new position reporting to the business technical head with the job of generating new opportunities for new uses of our products outside existing markets. This individual, in a few months, opened several promising avenues and killed a few sacred cows on his way, including many formal methods of idea generation. He did find several productive sources, however, and with aggressive follow-up, cold calls, and a small lab resource for scouting experiments now has several hot prospects. He has also tried several ideas and terminated them. He spends 50 percent of his time on the road.

"The net result of this activity is a number of needs-driven developments which are stimulating R&D projects promising enough to increase significantly the R&D resource in the business and to stretch current capability. Enough new business has already been generated to give confidence in the R&D potential. Other benefits are the stimulus provided to the sales and marketing personnel to broaden their horizons and, most important, to know that through this one individual they can excite resonance in R&D.

"The applicability of this action is the possibility for large organizations to create semiformal entrepreneurial mechanisms to develop new business for established product capabilities through appropriate structure, freedom, and choice of individuals. By granting a license outside the established markets to explore possible fruitful connections, the basic organization is not threatened. However, it is kept well-informed and is used where possible to reinforce specific opportunities."

Idea No. 8: Initiate a Commercial Development Function.

Source: Monsanto Company
Principal return component: operating efficiency
Principal activity: coupling to technical efforts—marketing and manufacturing

"The concept is to establish a 'commercial development' function which works collaboratively with all other operating functions within the corporation to 'shepherd' the flow of a project from research into fully established commercial success.

"The responsibility of the 'Commercial Development Manager' who is a project manager for one or more projects, is to work in a matrix fashion with all other functions—research, manufacturing, marketing, etc.—as business unit management, senior management, distribution, etc.—as required, to cause the new project to have optimum opportunity to be successful in the marketplace. The intended result is to focus otherwise diffuse corporate resources on an individual project and provide the project with the entrepreneurial attention of one individual who has accountability for moving the technology into the marketplace. It is not applicable where the project is of major importance to the corporation; under these circumstances a line project manager is generally a better alternative.

"The commercial development function also has a responsibility for identification of customer needs, and providing a "preresearch appraisal" of prospective new products prior to substantial research being undertaken. To accomplish this effectively, the Commercial Development Manager must have a strong technical background and be able to understand the technical capabilities of his or her own research organization and interact collaboratively with it. Credibility with the search organization is critical to effective operation in this role. He or she must also be able to interact collaboratively with it. Credibility with the research function is critical to effective customers at a technical level to elicit information on future market requirements far enough in advance of the need to permit his or her own research people to provide the necessary solutions.

"Commercial development functions are reasonably well established in the chemical and process industries; the concept may be useful in other industries as well. Information is available from the Commercial Development Association and in *Successful Commercial Chemical Development* (edited by H.M. Corley), and *Successful Product and Business Development* (edited by N.H. Giragosian)."

SOME THOUGHTS ON WHERE TO GO FROM HERE

This conceptual framework and its related set of definitions speak directly to the belief of I.R.I. representatives that there is tremendous potential for improvement in both R&D productivity and R&D yield. The R&D Productivity Subcommittee hopes that, by using these materials, individual I.R.I. representatives can boost the performance of their own organizations. Several suggestions for action have been described in the preceding section of this report, and these and others are summarized in Appendix B. In general, the Subcommittee hopes that I.R.I. representatives can utilize both framework and suggestions as the basis for a careful in-house rethinking of current practice. It would be presumptuous of the Subcommittee to offer a detailed plan for action—task forces, checklists, and the like—but a few "thought-starters" might be helpful and appropriate.

First, the principal value of this report will be as stimulus for thought and action in your own organization.

It can be used as a tool for gaining a common language between you and your peers in other functional departments; it can also be sent "up the line" to your CEO and shared with your R&D staff.

Second, you might use the Subcommittee's survey as the basis for conducting the survey among members of your own staff, your colleagues, even your CEO. If experience is a guide, thinking through the conceptual framework, definitions and list of activities will itself help to stimulate much discussion within your organization. You can also compare the results of your internal survey with those presented here.

Third, for the survey returns and discussions to help you analyze the potential for—and the best approaches to—improvement in your own organization, you will need to keep in mind:

- Your particular business or businesses, your products, your markets, etc.
- Your particular organization and responsibilities, for example, whether you are director of the central corporate R&D laboratory or the manager of product development for a single business unit or a smaller I.R.I. member company
- Your company's particular strengths and weaknesses—i.e., where the potential for improvement is the greatest.

Finally, the specific suggestions for action described both in the text and in Appendix B should help to stimulate similar ideas which would be most appropriate for your particular circumstance.

With the kind of thought and discussion these efforts should stimulate, we would hope that specific and positive action steps would not be far behind.

Appendix A

RETURN-IMPROVING ACTIVITIES

Appendix A provides details regarding two aspects of the return-improving activities discussed in the text. Immediately following are the definitions of 13 return-improving activities identified as most important, on average, by the surveys. Exhibits A-1 and A-2 display the complete set of 53 activities presented in the initial survey, from which these 13 were selected.

POTENTIAL PRODUCTIVITY

Identifying technical possibilities. The overall effectiveness of the system to identify and evaluate alternatives to present technical approaches. To the extent that a technology can be defined by its particular "limiting mechanism", i.e., different technologies have different limiting mechanisms—the identification of technical possibilities is the art of finding new technologies that circumvent old limiting mechanisms and of replacing them with new less restrictive limits.

Identifying limits. Identification of the "mechanism" or "mechanisms" that will bound the ultimate technical performance of the system. Such a "mechanism" may involve, for example, the need for a system's energy inputs, performance, outputs, and uses to be in balance (as in an automobile engine). In the case of catalytic restrictions, the number of active sites may limit the ultimate activity of the catalyst. Limiting mechanisms are more often of a "practical" nature, i.e., they are not defined by fundamental physical constraints but, rather, by supporting technology currently available. For example, auto engines are not today limited by Carnot efficiency but by the strength of materials at high temperatures in a combustion chamber. Large computer performance in the 1970's was not limited by the speed of an electron or even by the photolithographic process but by the ability to remove heat from the "packages" on which the chips were placed.

Characterizing technology. The process of determining how and why the product or process works as it does. Complete characterization implies understanding of all the physical phenomena that underlie product or process performance.

TECHNOLOGY-DEVELOPMENT EFFICIENCY

Professional personnel quality. A project team is only as good as its people. Better people result in better work at lower cost. Most companies believe they have good people, so it may be difficult to demonstrate convincingly that, on average and over time, one company has an intrinsic advantage over another in the quality of its people.

Identifying projects. The act of recognizing the need for a new technical project and specifying both what it should accomplish and how it should be approached.

Project staffing. Pulling together the necessary set of technical and managerial skills to get the job done.

Project planning. The ability to predict accurately the cost of time to reach a required objective or, alternatively, to specify what can be accomplished under certain costs and/or time constraints. Project planning also includes the ability to use this information to lay out a working plan in sufficient detail for everyone involved to know what he or she is supposed to do on a day-to-day basis as well as how to do it.

Project termination. The ability to: (1) reach a decision that the project is completed, no longer relevant, or unlikely to succeed either technically or economically; and (2) implement that decision. Often it is difficult to do so.

POTENTIAL YIELD

Demand/demand outlook. The process by which the sources of demand are identified and understood. "Understanding" here means knowing the value each market segment places on a product and the volume each segment will demand, both now and in the future. Demand needs to be understood as a function of the performance parameters of the product. Frequently, the demand for a product will increase with improved performance up to some level beyond which further improvements in product performance provide no further improvements in product value. The limited ability of the eye to see finer and finer picture quality is one example; the value of having softer and softer Kleenex is another.

Strategies of all competitors. Price is a strong function of capacity utilization in an industry. Thus it is important to consider the collective future strategy of present and possible future competitors. Companies often implicitly assume stable industrial structures, with the same competitors always being in the same game in the same way. Rarely is this the case; competition is much more dynamic, and this dynamism needs to be reflected in forecasts if profit potential is to be understood. Necessarily, these strategies should be expressed as a function of the level of technical performance.

OPERATING EFFICIENCY

Identification of customer needs. The process by which customer needs are spotted. The key to this step may well lie in understanding what the customer values now and is likely to value in the future. The customer will often have a good sense of what he values now but only a limited sense of what he will value in the future, that is, the company keeps supplying him as he is now being supplied. Thus, particularly where technical change is possible, managers need to understand what the customer will value under a variety of conditions, now and in the future.

Coupling to technical efforts—marketing. Not only must companies understand customer needs, they must express this understanding in terms that are meaningfull to the technical community. It is not sufficient for managers to say that they are looking for a "robust" typewriter; they must be able to say what "robust" means in technical terms so that R&D can do something about it. Since this communication is an intricate process and needs to be very precise (almost like a legal contract), it takes time and frequent interaction to do well. Moreover, since customer needs are constantly shifting, sometimes in subtle ways, the communication needs to be continual. And since the marketing department cannot be fully aware of changing technical possibilities for problem-solving, technical people need to have a direct link to end-users.

Coupling to technical efforts—manufacturing. The processes by which manufacturing requirements and what the manufacturing department "would like to have" are linked to what R&D can do. As with marketing, the essence of this activity is matching emerging needs with emerging solution possibilities. This matching frequently takes place best in the context of frequent meetings organized around specific issues.

Exhibit A-1

ACTIVITIES IMPROVING R&D PRODUCTIVITY

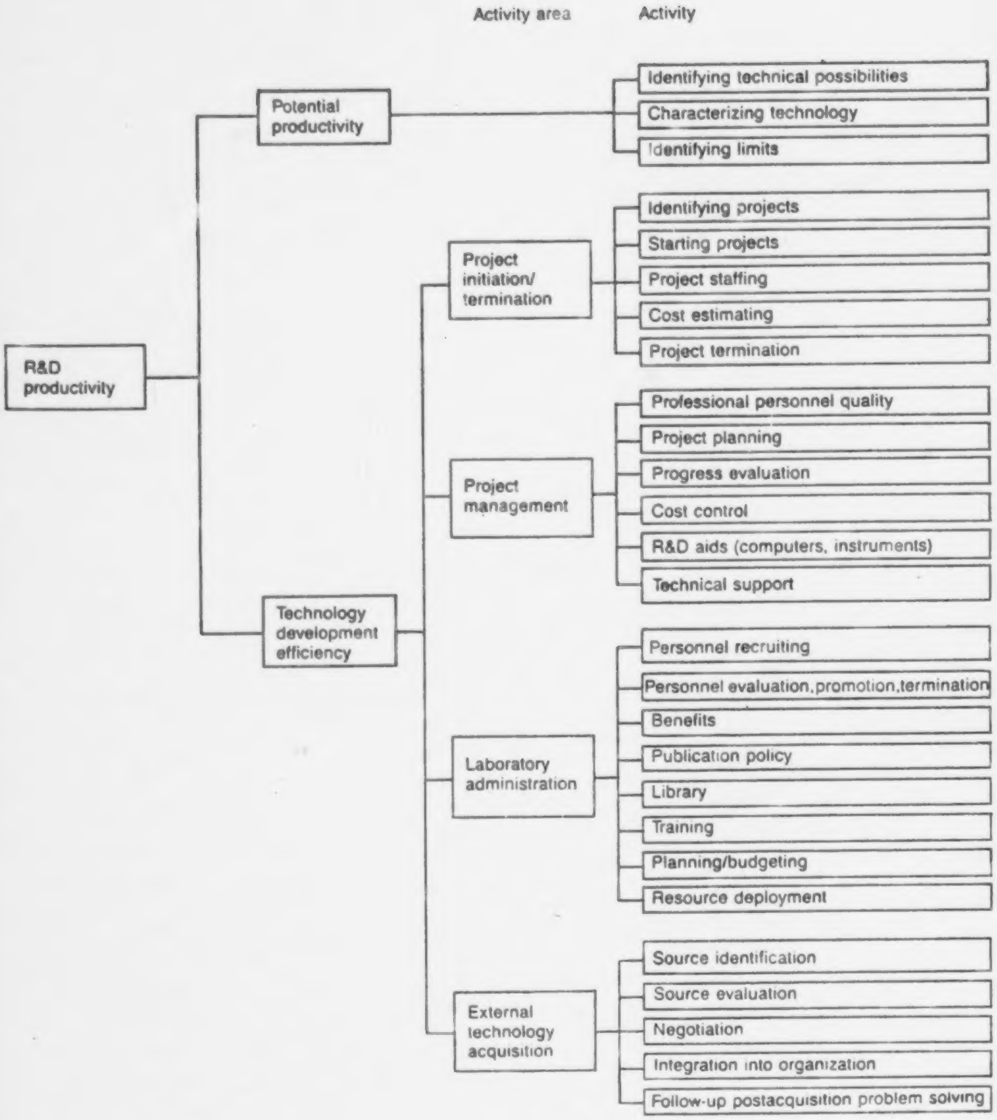
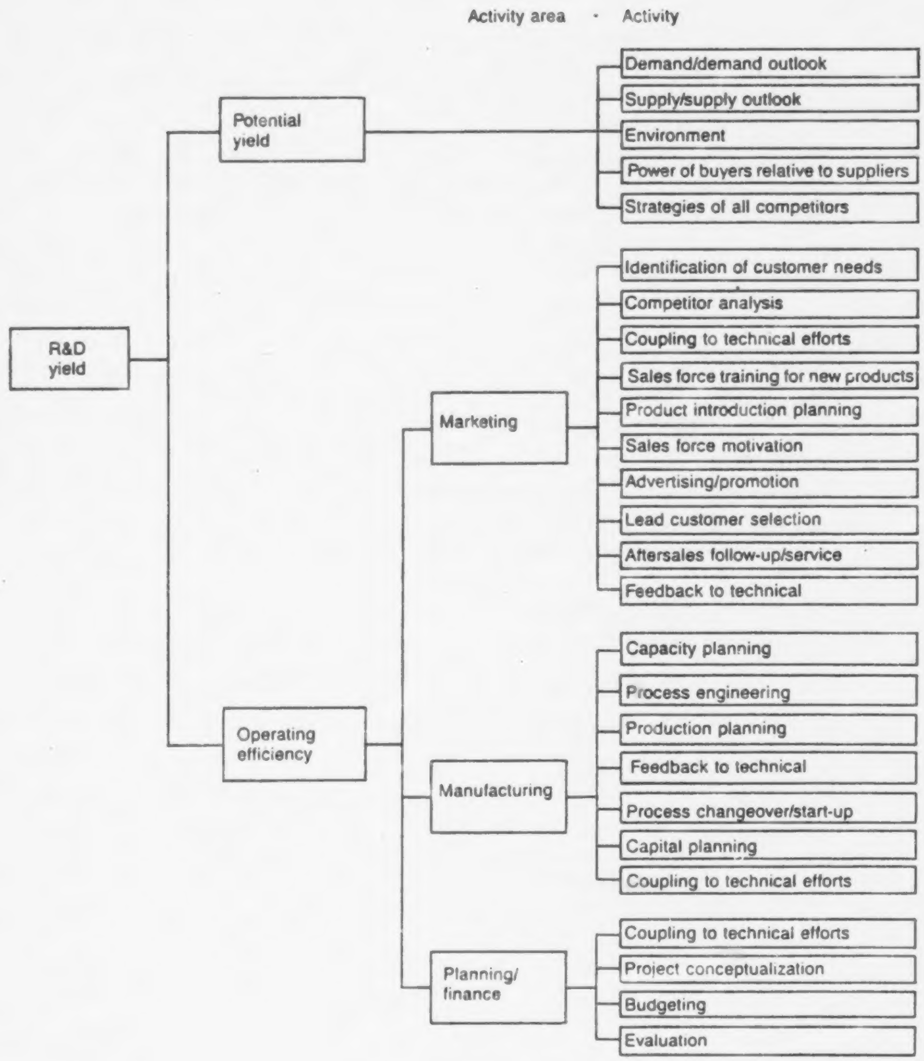

Activity area
Activity
R&D productivity
Potential productivity
Identifying technical possibilities
Characterizing technology
Identifying limits
Technology development efficiency
Project initiation/ termination
Identifying projects
Starting projects
Project staffing
Cost estimating
Project termination
Project management
Professional personnel quality
Project planning
Progress evaluation
Cost control
R&D aids (computers, instruments)
Technical support
Laboratory administration
Personnel recruiting
Personnel evaluation, promotion, termination
Benefits
Publication policy
Library
Training
Planning/budgeting
Resource deployment
External technology acquisition
Source identification
Source evaluation
Negotiation
Integration into organization
Follow-up postacquisition problem solving

Exhibit A-2

ACTIVITIES IMPROVING R&D YIELD


Activity area
Activity
R&D yield
Potential yield
Demand/demand outlook
Supply/supply outlook
Environment
Power of buyers relative to suppliers
Strategies of all competitors
Operating efficiency
Marketing
Identification of customer needs
Competitor analysis
Coupling to technical efforts
Sales force training for new products
Product introduction planning
Sales force motivation
Advertising/promotion
Lead customer selection
Aftersales follow-up/service
Feedback to technical
Manufacturing
Capacity planning
Process engineering
Production planning
Feedback to technical
Process changeover/start-up
Capital planning
Coupling to technical efforts
Planning/finance
Coupling to technical efforts
Project conceptualization
Budgeting
Evaluation

Appendix B

SUGGESTIONS FOR IMPROVEMENT OF R&D RETURN

POTENTIAL PRODUCTIVITY

Identifying Technical Possibilities

- Technology Gatekeeper (Allied). Appoint an outstanding scientist who exhibits "generalist" tendencies as Chief Scientist and give him or her a broad charter to assist technical program managers and to act as a technology "gatekeeper."
- Formal Idea Generation and Screening Program (Weyerhaeuser). Start a systematic program to generate and screen new business ideas. The program could include structured idea-generating techniques, a future-oriented set of descriptive scenarios to serve as a "mind-expanding" tool, a set of idea-screening criteria, and a programmed series of steps for evaluating ideas as they progress through R&D.

Identifying Limits

- Selection of Clear-Cut Advantages (American Cyanamid). Identify technology limits to put greater emphasis on R&D efforts that would lead to clear-cut advantages in the marketplace.
- Location of Unit Operations on "S-curve" (Engelhard). Analyze unit operations individually to locate them on the S-curve, so as to identify potential for improvement. Process changes can lead to new products.
- Limits Identification for R&D Planning (Owens-Corning Fiberglass). Determine key technical performance factors which affect product value, and technical limits to those factors, for product and competition. This will point out useful areas for focus of research and give insights into competitor's technical position.

Characterizing Technology

- Research Stimulation (General Electric). Establish a program of in-house grants for basic research. Grants can be made annually to young scientists and engineers, often new to the organization, to undertake promising fundamental work in new areas of technology. Select by a peer group from proposals submitted, and must be work that falls outside normal program priorities.

TECHNOLOGY DEVELOPMENT EFFICIENCY

Professional Personnel Quality

- Improvements in Recruiting (American Cyanamid). Enhance research reputation through increased publication and small grants to universities to improve recruiting.
- Incentives and Academic Ties (Merck). Maintain incentives for career development internally, but also ensure adequate flows of new people at all levels. Maintenance of academic ties is also important to enrich the intellectual environment. (RESEARCH MANAGEMENT, November 1979, pp. 15-18).

Identifying Projects

- Peer Groups to Allocate Research Funding (Northern Telecommunications). Require laboratory researchers to "sell" research project ideas—on the basis of both product development usefulness and probability of technical success—to peer groups with funding control.
- Systematic R&D Return Estimation for New Opportunities (Abex). Divide R&D projects into "Opportunity Modules" and "Base Case." For Opportunity Modules, estimate all incremental costs and margin dollars over 5 or 10 years; if ratio of margin total to cost total is less than one, reject the project. Rank order the rest to assist in resource allocation. 'We found that 60 percent of proposed projects were rejected, but that the fraction of sales due to new products in the division most rigorously using this method was doubled since 1977 with the same R&D to sales ratio.'
- Technology Catalog (Rohm and Haas). Develop and regularly review a catalog of technologies important to each major product line. Evaluate importance of each new technology, corporate position in the technology, how the technology is changing, and likely competitive impact. Use the results to evaluate strategy options.

Project Staffing

- Persistent Dedicated Leader (GTE). Emphasize the importance of a persistent, dedicated project leader—not necessarily the most intelligent or technically knowledgeable person, but one who is an effective manager capable of understanding the issues and setting up programs for solving problems.
- Specially Selected R&D Task Teams (Manville). Set up short-term multidepartment task teams to solve R&D problems, with special attention to the qualities of individuals as well as the problem-solving process.

Project Planning

- Satellite Laboratories (RCA). Co-locate central R&D groups with operating division engineering functions and conduct joint project reviews to ensure consideration of all key business factors in R&D project plans.
- Diagnostic Tool to Evaluate Linkage of Technical Projects to Business Unit Strategies (Owens-Illinois). Inventory technical projects by business unit, project spending, business objective, and technical content; define expectations of business units; analyze and evaluate.
- Planning Process Focusing on Probability of Success Given Constraints (Uniroyal). Utilize a four-step process including assignment of a probability of technical success, a probability of marketing success, estimation of all relevant research costs, comparison with ongoing projects. Then set up a PERT-type simplified management tracking program.
- Formal Model for Tracking Product Performance Changes over Time (GTE). Based on a literature review and feasibility assessment, an exponential model of progress as a function of independent factors (time, volume produced, etc.) can be used to track, predict, and communicate product performance.
- Tracking Projects to Highlight and Modify Plans (Leeds & Northrup). Generate a project plan. Review at appropriate, not at regular intervals. At each review, set the date for the next and the expected accomplishments. Grade each review, in MBO fashion, a hit or a miss. Overall project graded half in this way and half on meeting the overall objectives on time and on budget. This system allows the flexibility needed in research programs, provides for participation in planning by employees, and is easily melded into an MBO program.
- Study of Key Planning Factors (3M). Study past success and failure factors to provide the basis for improved selection of R&D goals.
- Statistical Planning (SPS Technologies). Utilize statistical planning of experiments and programs to increase the amount of "bang for the buck." This procedure can significantly reduce the number of experiments, thereby reducing cost and time to completion. Furthermore, interaction effects of variables may be determined more readily and the probability of missing peak points is virtually eliminated.
- Joggers and Runners (General Electric). Categorize projects as "runners" which are not limited by technology resources, and "joggers" which are. This communicates to management in non-R&D departments whether or not a project is being pursued with maximum speed by R&D.

Project Termination

- Threshold Parameters (Kendall Co. and others). Terminate projects in an orderly manner by having the project performance parameters well defined and comparing them to a threshold of minimum acceptability. (RESEARCH MANAGEMENT, November 1981, p. 7).

POTENTIAL YIELD

Demand/Demand Outlook

- R&D Project Post Audits (Mobil). Calculate a research-return ratio annually for each project, with emphasis on future demand and business risks. This system serves to motivate researchers, demonstrates laboratory productivity, identifies most productive research areas, and increases confidence in predictive evaluations. (RESEARCH MANAGEMENT, January 1978, pp. 28-30).
- Major Accomplishment Reporting (Alcoa). Review the prospective economic impact on a quarterly basis of completed R&D projects. This serves to reveal anticipated benefits that do not materialize and so brings into focus realistic accomplishments and aids the implementation of high-priority projects. (RESEARCH MANAGEMENT, March-April 1983, pp. 23-27).

Strategies of All Competitors

- Competitve Gap Analysis (Mead). Define the technical status of your products and processes in comparison to the best elements of the competition; a negative gap could form the basis for productive R&D projects. Sources of competitive data include direct product analysis, trade journals, discussions with suppliers, etc.; it is important that these data be formally evaluated and melded to define gaps. Equally important is a program to define and create positive gaps.

OPERATING EFFICIENCY

Identifying Customer Needs

- Individual Responsible for Identifying New Uses for Products (Polysar). Appoint an individual with a small lab resource to make technical evaluations and a charter to make cold customer calls with aggressive follow-up to produce new business and also new ideas for R&D projects.
- Technical Selling (Armco). Maintain on the R&D staff individuals who are expert in the customers' area of application and who can "talk the language" of the customer. These people should work with field sales in customer contacts and develop projects to satisfy customer needs.

- Customer Survey by Field Sales Staff (Kaiser Aluminum and Chemical). Use field sales staff to regularly survey major accounts in key markets for data on technical breakthroughs affecting their business, or problems where technical expertise could be applied. Screen and submit results for review by multi-department advisory committee.

Coupling to Technical Efforts—Marketing and Manufacturing

- Formal New Product Process (GTE). Use a standardized, well-documented process with manufacturing and marketing sign-offs.
- Interdepartmental Staffing of Projects (Eastman Kodak). Assign manufacturing engineers as well as researchers and development engineers to advance technology projects to create earlier a sense of ownership in the technology and credibility with the product development and manufacturing organizations.
- Continuity of R&D Teams (Ethyl). Use joint staffing and carry-over of personnel from one stage to another to facilitate the systematic breaking down of divisions between research, development, pilot plant, and plant design.
- Long-range Plan Coordination (Kellogg). Undertake formal coordination of long-range technology plan with marketing plan and operations plan.
- Technical Request Program (Siecor). Work with marketing to formalize its "marketing needs" in the form of "technical requests" to R&D, to agree on needs and to link individuals between departments.
- Research Liaison System (Armco). Designate one researcher to have principal responsibility for interfacing with each division or business.
- Joint Task Force (Sohio). Appoint a task force of business unit management, research, and marketing personnel, to set priorities jointly based on what is marketable and what is doable. The work of the group can be facilitated through use of a "retreat" away from normal responsibilities.
- Focus Projects (Deere). Establish "focus projects" involving Technical and Operating units with tightly specified commercial objectives and target dates.
- Technology Forecasting (Deere). Formalize technology forecasting by Technical Center for use in corporate strategic planning. This results in better plans and also better and more frequent contact between technical Center and business units.
- Research Liason Group (Leeds & Northrup). "Just as baseball pitchers are not outstanding hitters, good researchers are not necessarily outstanding communicators. A separate group, dedicated to finding new applications and increased utilization of technical advances made in

research, can fill this gap. Personnel in this organization are more market-oriented, and picked for their communication skills as well as an ability to at least understand the technology. They also can play a key role in determining the *objectives* for research, but not the approach to achieve those objectives.”

- Payback Development Project (Rexnord). Develop a written agreement between corporate R&D and the operating division specifying the prospective product, with the division agreeing to payback-development costs plus a profit to R&D from a fixed percentage of sales of the new product.
- Resource Commitments from non-R&D Functions (Uniroyal). Require R&D group to: (1) estimate time and resource commitments necessary from non-R&D groups (engineering, manufacturing, etc.) for project success; (2) prioritize them together with other groups; and (3) get other groups to "buy off" on the commitments.
- Regular Monitoring of Manufacturing Processes (U.S. Steel). Develop with manufacturing simple, mutually-agreed numerical parameters which tie R&D's understanding of the process to operating numbers of interest to manufacturing. With process monitoring, use insights to generate suggestions for incremental improvement and manufacturing efficiency.
- Commercial Development Function (Monsanto). Start a commercial development function, which would work matrix-style with other corporate functions to move a technology or product concept into the marketplace.

DISCUSSION

Mr. **Fuqua**. Thank you very much, Dr. Foster.

What kind of return on investment can be used by Congress, say, in choosing research areas that it thinks are important and need support?

Dr. **Foster**. I would think the answer to that question would vary from agency to agency. The real question that you're asking is what measure of benefit. In industry, a return clearly means profit. In the Government, benefit does not necessarily mean profit at all.

I think we need to ask what is the benefit to society of our medical research; what is the benefit to society of our defense work; what is the benefit to society of our environmental work? If we can come to grips with those questions, then I can easily tell you what the measures of returns on the investment are. I believe those questions are fairly difficult questions to answer and are ones that have been wrestled with for some time.

Mr. **Fuqua**. Do you think that each of the agencies should try to describe or set forth areas that they deem are critical?

Dr. **Foster**. Yes, sir, that's exactly what I think. And I think they should very much think about who their users are, to the extent that——

Mr. **Fuqua**. I'm speaking now of, say—let's talk about basic research rather than applied. Applied would probably have a user community at some point. But if you're talking about basic research, which we get involved in in supporting——

Dr. **Foster**. Let me chat a bit formally about basic research. I covered a bit of that in the paper here but I chose not to talk about it in the formal testimony.

The purpose of development is to develop a product which is useful to someone in the marketplace. The purpose of research is to provide the knowledge necessary to design that product. The purpose of research is not to design the product, per se, but rather to let us know whether we can and should define that or not. Therefore, it is not relevant to measure research in terms of the benefits that we have been talking about here. Rather, it is relevant to measure research in terms of the knowledge that is needed that it provides. We can talk about the efficiency of that, but the relevance has to do with the relevance of the user, and the value of that information to the user.

Mr. **Fuqua**. To further expand on that question, we had a previous witness—and you're alluding to that now—that new mechanisms must be invented to promote technology from basic research. I personally feel that's very important. But how do you view the Government and industry role in trying to accomplish this?

Dr. **Foster**. I think we need a good deal more basic research in this country. I think we are no place near diminishing returns in our investment there. It seems to me that the universities offer a very important intermediary role between the Government and industry in providing that knowledge. Industry is not well suited to provide the knowledge that they themselves need. The mechanisms for industry-university cooperation are not strong enough yet to provide the extent of interaction that we would like. I would encourage you to encourage the departments to figure out better ways to have industries and universities work together through the elimination of legal constraints, more adequate funding of university research aimed at industrial purposes. Those would be the areas that I would focus on.

Mr. **Fuqua**. Coming from industry, how do you feel about, say, in the area of big science, that it be on a more international basis, and in that particular area you maybe lose a certain amount of nationalism in promoting that research, and you may not always be No. 1 in that area? But when you're talking about big ticket items, is that possible, or would that have an adverse impact on American industry?

Dr. **Foster**. It certainly is possible, and certainly we're doing that today. As you may know, I'm on the Technical Advisory Board at Oak Ridge National Laboratory, and even now, with the current fiscal constraints here in the Government, there are substantial cutbacks occurring in Oak Ridge. Part of the response is to work with the Japanese, for example, in areas of nuclear energy, and so forth. I must confess to some mixed feelings on that. I'm not at all sure that's a good idea. I am not sure we are valuing the option of our independence highly enough. In some cases, I would rather see us not do anything at all in an area, and take those funds and put it into an area where we can make progress, than to kind of cut back the efforts and to get support from other countries. It is a very difficult question, frankly, for me to deal with. But I do have nervousness about some of these deals that we're looking at with the Japanese right now

Mr. **Fuqua**. Thank you very much, Dr. Foster. We are grateful to you for your sharing with us this morning your thoughts.

Dr. **Foster**. Thank you.

Mr. **Fuqua**. Our next witness will be Dr. Eltinge, director of research at the Eaton Corp.

[A biographical sketch of Dr. Eltinge follows:]

DR. LAMONT ELTINGE

Dr. Lamont Eltinge was born and raised in Chicago. He is trained in mechanical engineering. HIs undergraduate study was at the University of Oklahoma and at Purdue University, where he received a bachelor's degree in mechanical engineering. His graduate training at the Illinois Institute of Technology led to a master's degree and doctorate in mechanical and aerospace engineering.

He has worked in manufacturing at the Electro-Motive Division of General Motors in Illinois. He's worked in industrial research in Indiana and Michigan. His experience has been predominantly in the engine, fuel, energy, and ecology fields. In the research and development department of the Standard Oil Company of Indiana, he worked on aviation, motor and diesel fuels to improve the performance, durability, and economics of the engine/fuel combination. He directed automotive research for the Ethyl Corporation; there, major emphasis was on engine modifications to achieve clean exhaust with economic and available gasoline. As vice president of research and technology for Cummins Engine Company, he was responsible for the development and application of advanced specialized technology that would improve the competitive performance, durability, and cost of that company's heavy-duty truck engines, and identify and develop engines that would meet future requirements competitively and better than current engines.

He joined Eaton Corporation in mid-1973. His attention as director of research is focused upon the identification and assimilation of new technologies that, when reflected in Eaton's operations, will help make the products better and the businesses healthier and larger.

He's married, has a daughter and a son, and lives in Northville, Michigan. He participates in Society of Automotive Engineers, Industrial Research Institute, and

ASME activities. He also participates in several church and society organizations. He is a member of the industrial advisory boards of Bradley State University, Oakland University, Wayne State University, and the University of Virginia.

STATEMENT OF DR. LAMONT ELTINGE, DIRECTOR OF RESEARCH, EATON CORP., SOUTHFIELD, MI

Dr. **Eltinge**. Mr. Fuqua, for the record I'm Lamont Eltinge. I am the director of research for the Eaton Corp.

I should comment that while I am employed by Eaton and value the experience I have received working for them—and also, as you know, because we met there at the IRI meeting almost a year ago, I have been very much involved and have benefited from the exchange of views and ideas, insights, with my peers in the industrial research community through IRI—neither of those is responsible for anything I say. These are my own personal views. I hope they are of some use to you because I think the topic that you're addressing is of great importance to the Nation and there is a role for the Congress and the Government to play in fostering basic research, applied research, and its effective application to the benefit of the Nation.

I am assuming from the staff report that your focus is on the things that Congress can do to act in its usual four ways to effectively contribute in this area of basic research. I think those four things you can do. One is to focus attention on the subject—certainly you're doing that today—and in other ways. Second, you can direct resources, either directly or indirectly, to basic research and its use. Third, you can codify the relationships in laws for the most productive way that the pieces of our Nation can work together in that job. And finally—and you were just dealing with it—is the issue of how we interact with those in other nations to our benefit, hopefully to the benefit of all.

I think you will find that my comments you might regard more as tactical than strategic. I am speaking from a number of years experience involved, if you would, in the trenches, of trying to find the results of good research and put them to good use for the company that I was part of at any particular time.

I would agree with some things that show up in the OTA, and your staff reports, that basic research is important. I would agree with Dick as well, that there is a need, I believe, for more of it. But it needs to be the correct research and it needs to be well guided and effective. I use the word "guide," rather than "direct" or "manage," because research has to have a great deal of freedom; but that is not a chaotic freedom. There ought to be a sense of self-discipline and a self-guidance with it, and I think there is something that the Congress can do there.

There is one other thing that your report does show that I think would be worthwhile paying some attention to, and that is increasingly true, and that is that knowledge, well-disciplined, new information, is increasingly a critical resource and a critical asset. In terms of the Nation, it is the knowledge that we have and apply well that probably more than anything else is the resource that will determine our future. In terms of a company, we look at the knowledge as an asset. Unfortunately, we don't show it on our books, but that doesn't keep it from, probably, being the most important asset we have. If you have good knowledge, the people who have it do well; if you don't, you're in trouble.

It might be worthwhile to look at the system as an engineer would. An engineer looks at a system and he says, "Boss, what do you have in mind? What do you want to achieve? What's the goal?" Second, he has some understanding of the system that you're dealing with, the principles. You then make some measurements of how you're doing; you have some ideas of actions you might take; and then you make measurements as you go, in a closed-loop mode to see whether the actions you have taken yield the results you want.

I believe that your call for your hearings asked what can we do in the way of effective measurements. I think you will find that, looking at it from the trenches, one sees not global dollar measures but some other things that may be useful to you, to the Congress, as you try to carry out your work. I hope so.

First of all, let me describe the system that we're dealing with, at least from my perspective—and certainly there are many others. One is that we're dealing with a system that starts with basic or applied research. It also starts with understanding a need and an idea of how to use the knowledge to meet the need. We add to this a great deal of effort in development; we put in place capital investment; we have a lot of people who work hard at it; and we tie money up in the process. Hopefully, when we've gotten all that done well, we have grown a very healthy orchard and we're producing good fruit from the orchard, and that serves the Nation well.

I would point out that what we're talking about here is really just the seed, the basic research and the knowledge. It is a key resource, it's scarce, it's limited. But the delightful thing is that the more research you do, the more good research you do, the more you involve young people in doing it and train them to do it well, the greater your capacity as a nation to do research. That is worth keeping in mind.

In addition, a second idea that you ought to have in mind as you're trying to set your plans is that knowledge resides not in books, not on paper, but it resides in expert people who are knowledgeable and disciplined and dedicated to the field. You, therefore, want to have the individuals in mind.

Third, the process of going from the seed of research to the end product, that we all as citizens value is slow. It is somewhat irratic. There is a lag of 5, 10, 20 years. Imagine the work that Shockley and Bardeen did at Bell Labs that led to the transistor, that led to the integrated circuit, that led to the big supercomputers or your pocket calculator. That was more than 25 years ago. It was more than 35 years ago, and we haven't seen the end yet.

I guess what we would like to do is start another productive chain like that. That would be great. And that's really what we're trying to do. It is too late to wait until we measure in dollar terms the final result to know that we're doing well, if we're going to, as engineers, control the system effectively, to make sure that we are planting the right seeds and treating them well. You do need to deal with that.

Finally, it is important that the pieces of that system interact.

Let me make one other observation and then suggest some things that might be done, and that is something out of either science or engineering, the Heisenberg principle. It says you can't measure something without influencing it. That is true of physical systems, and it is particularly true of a social system, which is what we're talking about here. I think the important thing to do is to make sure that the measures that you use are positive in their effect. You not only know what's going on, but the very fact that you're measuring it makes the system function better.

Let me suggest, then, some things you might do and how you might measure them.

First, certainly you want to focus attention on the subject, as you are doing here. One measure might just simply be the amount of attention that gets given to this in Congress, in the agencies of Government, and in the public sphere. You can count those, but you can't count them in dollars.

Second, it is important, recognizing that people are important, that as you fund research you ask who is doing the research and, are their expectations to stay in the field so that they will be the vessel in which the knowledge that we the people have paid for gets used for our benefit. Now, that can't be a guarantee, but it can be an expectation. And then it is worthwhile, retrospectively, to say, "How valid are those expectations as an indicator of what's going to happen?"

Third, we are interested in whether you couple the basic research into what goes on downstream in that chain. I think there are two things that can be done there, and they are somewhat related. The thing you want to see is that there is coupling of the researchers with, interestingly, researchers in other disciplines.

I think you would find it very interesting to walk on to a campus and just, "spot-sample". Walk into a department of electrical engineering and say, "Have you talked to anybody over at the biology school? When last?" I think you would find we could stimulate that kind of coupling.

More important as well, and easier to measure, is the coupling between the university and industry. Three measures might be used of such coupling.

One is, did U.S. industry hire the graduate students from that particular program to work in their laboratories or elsewhere in their business? That is where you really couple.

Second, measured a little more subtly, is to ask: "Have you been on that campus; have those people been in your laboratory talking about the subject of the research?" That's a very effective measure.

Third, ask people in industry: "Are you doing work in your laboratory that is closely coupled to the work being done under Government funding in the university? Or are you funding work in universities?" Those are quantitative. Those happen to be dollar indicators. More important is the number of man-years involved.

There is something that you might do—and given the current budgetary attention, I am somewhat reluctant to suggest it, but I think it might be worthwhile—you might consider some form of allowance or tax credit for the industry funds that were spent on research at universities. And that has to be basic research, or at least applied. It cannot be development because that wouldn't serve the goal. But to allow a tax credit of some form for that research. I would not recommend that you make it completely costless to the industry or to the company because you really want them to express in their use of their funds their best judgment that while this is long term and risky and all of those things, it really is a good bet, because that's the kind of judgment you want. You can't get that judgment from the gross numbers. You have got to get that judgment down in the trenches, eyeball-to-eyeball, because the key to the process is coupling of the individual researcher and the user of the results of that research. That has to flow through all of this.

Finally, there is something you might want to debate in Congress. Knowledge is going to be the key asset. As a country, I think we have pioneered in the patent system, where we said inventiveness—and that's different from the knowledge that comes out of research—we have said to the inventor, "If you will reveal your idea, disclose it to the Patent Office so that it's published, we will give you an exclusive right to the use of that invention for a fixed period of time." I think that that kind of principle could be extended to research because we're dealing here with knowledge; we want to stimulate it; we want individuals with a great variety of different viewpoints, to commit their own resources to stimulating it. You might extend the patent principle to the results of basic research granting exclusive use of them for a limited time, when it then becomes a part of the body of knowledge available to all.

Finally, something that needs to be dealt with is the relationship between research in the United States and elsewhere. There's a dilemma. If you say we won't talk to anybody else, we shut ourselves off from a great deal of knowledge—more than half. That wouldn't be smart. At the same time, if we completely give away everything we do, that isn't the optimum, either. Like most things that are important, you need to strike as fine balance. I think what you want to do is establish the basic principle that the flow of information both ways should be equal, that's a good measure. Remember that information flows in people. One kind of measurement you might use is that the number of graduate students from any foreign country in the United States ought to be equal to the number of U.S. graduate students in that foreign country. Then you end up with a two-way flow of information fairly well assured. Precisely? No. Fairly good? Yes.

With that, let me stop. I thank you very much for your time and attention. I hope there's an idea or two there that might be useful. You have the printed testimony, for whatever help that may be. I will try and answer any questions you may have.

[The prepared statement Dr. Eltinge follows:]

TESTIMONY BY LAMONT ELTINGE
BEFORE THE SCIENCE POLICY STUDY TASK FORCE OF THE U.S. HOUSE OF REPRESENTATIVES COMMITTEE ON SCIENCE AND TECHNOLOGY - 30 APRIL 1986
ON "RESEARCH FUNDING AS AN INVESTMENT"

MR. CHAIRMAN AND MEMBERS OF THE TASK FORCE, FOR THE RECORD, I AM LAMONT ELTINGE, DIRECTOR OF RESEARCH FOR EATON CORPORATION. I AM INDEBTED TO EATON FOR ALLOWING ME TO TESTIFY AND FOR SUPPORTING AND FUNDING THE EXPERIENCE THAT IS PART OF THE BACKGROUND FOR MY TESTIMONY. I AM ALSO INDEBTED TO THE INDUSTRIAL RESEARCH INSTITUTE (I.R.I.). HOWEVER, NEITHER IS RESPONSIBLE FOR THE VIEWS I EXPRESS; I, ALONE, AM RESPONSIBLE FOR THOSE AND I THANK YOU FOR THE OPPORTUNITY TO PRESENT THEM.

TODAY'S TOPIC, THE SUPPORT AND GUIDANCE OF RESEARCH AND THE TREATMENT OF THE KNOWLEDGE RESULTING FROM THAT INVESTMENT AS A VALUABLE ASSET, IS OF GREAT IMPORTANCE. THE OFFICE OF TECHNOLOGY ASSESSMENT FINAL DRAFT REPORT ON "RESEARCH FUNDING AS AN INVESTMENT; CAN WE MEASURE THE RETURNS?" OF MARCH 19, 1986, AND THE REPORT BY THE TASK FORCE ON SCIENCE

POLICY, "AN AGENDA FOR STUDY OF GOVERNMENT SCIENCE POLICY, DECEMBER 1984" REFLECT THE SERIOUS THOUGHT THAT HAS ALREADY GONE INTO YOUR DELIBERATIONS. I AM HONORED TO HAVE BEEN INVITED TO SPEAK TO YOU AND I AM AWED BY THE CREDENTIALS OF THE OTHERS WHO ARE TO TESTIFY. BUT, PERHAPS I CAN ADD THE PERSPECTIVE OF THOSE DIRECTLY AND DEEPLY INVOLVED IN TRYING TO DEVELOP NEW KNOWLEDGE AND TO APPLY IT FOR USEFUL PURPOSE.

A SCIENTIST OR ENGINEER RECOGNIZES THAT THE INFORMATION HE OR SHE RECEIVES IS DEPENDENT ON THE INSTRUMENT USED TO OBTAIN IT. MY TESTIMONY SHOULD BE INTERPRETED AGAINST SOME CHARACTERIZATION OF ME AS YOUR INSTRUMENT. AS YOU WILL NOTE FROM MY BIOGRAPHICAL SKETCH, I HAVE ABOUT 40 YEARS EXPERIENCE, ALMOST ENTIRELY IN INDUSTRY, TRYING TO COST-EFFECTIVELY DEVELOP THE BEST RESEARCH RESULTS AND TO GET THEM USED FOR THE BENEFIT OF THE ORGANIZATIONS THAT SUPPORTED THE WORK. THAT EXPERIENCE BASE HAS BEEN STRENGTHENED BY THE SHARING BY OTHERS OF THEIR SIMILAR EXPERIENCES THROUGH I.R.I. THAT IS NOT THE ENTIRE SCOPE OF YOUR STUDY, FOR IT INCLUDES HEALTH, DEFENSE, AND CULTURE. IT SEEMS BEST TO LIMIT MY TESTIMONY TO THAT SUBJECT MATTER IN WHICH I AM A VALID INSTRUMENT FOR YOU AND I WILL DO SO.

AN ENGINEER ALSO KNOWS THE OBJECTIVE OF A PROJECT AND GUIDES HIS EFFORTS TO ACQUIRE AND PROVIDE DATA PERTINENT TO THE DECISION AND ACTIONS NECESSARY TO ACHIEVE THOSE GOALS. AS I UNDERSTAND IT, ONE OF YOUR MAJOR GOALS IS TO STIMULATE THE GENERATION OF BASIC RESEARCH RESULTS AND TO PROMOTE THEIR USE TO ENHANCE THE HEALTH, VIGOR, AND GROWTH OF U.S. INDUSTRY AND ITS COMPETITIVENESS IN AN EVER-MORE CHALLENGING WORLD MARKET. YOU SEEK TO WISELY GUIDE A SMALL FRACTION OF THE U.S.' CURRENT RESOURCES INTO RESEARCH LIKELY TO PRODUCE RESULTS THAT WILL RETURN TENFOLD BENEFITS TO THE U.S. IN THE FORESEEABLE FUTURE. AND YOU SEEK TO IDENTIFY MEASURES OF RESEARCH THAT YOU CAN USE IN YOUR OVERSIGHT AND ALLOCATION OF RESOURCES TO SUCH RESEARCH. AND ITS SUBSEQUENT TRANSLATION INTO VIGOR OF THE U.S.

IN SIMPLE TERMS, YOU, THE CONGRESS, MAKE YOUR CONTRIBUTION TO THE TOTAL U.S. EFFORT BY:

1. FOCUSING ATTENTION ON CRITICAL ISSUES; YOU ARE DOING THAT WITH THESE HEARINGS, AND I HOPE YOU WILL CONTINUE AND EXPAND THAT EFFORT;

2. OBTAINING RESOURCES THROUGH TAXATION AND ALLOCATING THEM TO PROGRAMS;

3. CODIFYING, IN LAW, THOSE RELATIONSHIPS BETWEEN INDIVIDUAL AND/OR CORPORATE CITIZENS THAT WILL MAKE OUR SOCIETY MOST EFFECTIVE; AND

4. ESTABLISHING AND GUIDING THE RELATIONSHIPS WITH OTHER COUNTRIES THAT WILL BEST SERVE OUR NATIONAL INTEREST.

I TRUST THIS TESTIMONY WILL HELP YOU DO THAT AS WELL AS YOU WANT TO.

IT MIGHT SERVE YOUR PURPOSE TO HAVE SOME OBSERVATIONS ABOUT IMPORTANT CHARACTERISTICS OF RESEARCH AND ITS TRANSLATION INTO BENEFITS FOR THE COUNTRY AS A WHOLE. YOU ALSO APPEAR TO SEEK IDEAS ABOUT ACTIONS YOU MIGHT TAKE AND MEASURES OF RESEARCH EFFECTIVENESS THAT YOU MIGHT USE IN YOUR OVERSIGHT.

THE TOTAL PROCESS OF CREATING NEW KNOWLEDGE AND USING IT TO CREATE MORE USEFUL GOODS AND SERVICES, A HEALTHY AND GROWING ECONOMY, AND INDUSTRY THAT CAN COMPETE IN INCREASINGLY SEVERE WORLDWIDE COMPETITION IS IMPORTANT. IT'S ALSO EXCITING AND FUN.

BASIC RESEARCH IS A SMALL BUT KEY PART OF THAT PROCESS. IT IS LESS THAN 1% OF THE TOTAL ECONOMY. YET, IT IS THE "SEED" FROM WHICH FUTURE, HEALTHY ENTERPRISE AND ECONOMY WILL GROW. IT IS MEANT TO CREATE VALUABLE AND VITAL KNOWLEDGE.

KNOWLEDGE IS THE KEY ASSET IN TODAY'S COMPETITIVE, INDUSTRIAL WORLD. IT IS THE PROPERLY ASSEMBLED AND ORGANIZED BODY OF NEW INFORMATION THAT CAN BE EFFECTIVELY USED TO CREATIVELY AND INNOVATIVELY PRODUCE NEW PRODUCTS AND SERVICES. ONE LEARNS THROUGH EXPERIENCE THAT SUCH KNOWLEDGE IS LARGELY EMBODIED IN PEOPLE, AND IT DIFFUSES AROUND THE WORLD AT A RATHER RAPID PACE. AND, IT DIFFUSES WITHIN THE U.S. AND ACROSS OUR BOUNDARIES. THE DIFFUSION OF RESEARCH RESULTS TO U.S. INDUSTRY AND ITS USE OF THEM SHOULD BE ACCELERATED; THE FLOW OUT OF THE U.S. OF THE RESEARCH RESULTS THE U.S. TAXPAYER PAYS FOR SHOULD BE SLOWED. SUCH DIFFUSION IN PEOPLE OCCURS LARGELY; IT NEEDS TO BE REFLECTED IN THE MEASURES YOU USE AND THE ACTIONS YOU TAKE.

TOTALLY, FOR THE U.S., THE STREAM OF RESEARCH, PRODUCT DEVELOPMENT, AND ECONOMIC ACTIVITY, IS A COMPOSITE OF MANY, RELATIVELY SMALL, INDEPENDENT YET

INTERACTING PIECES. FOR ANY ONE PIECE, SUCH AS FROM SHOCKLEY ET AL AT BELL LABS TO TODAY'S SEMICONDUCTOR, ELECTRONIC, AND COMPUTER INDUSTRIES, THERE IS A LEAD TIME OF TEN TO TWENTY YEARS BETWEEN THE CONDUCT OF THE RESEARCH AND MATURE PRACTICE AND ECONOMIC BENEFIT. IT IS IMPORTANT TO RECOGNIZE AND ALLOW FOR THAT LAG. WE CAN DO SOME THINGS TO SHORTEN IT AND SHOULD, BUT IT WILL STILL BE VERY SUBSTANTIAL. YOU CAN HELP BY RECOGNIZING THAT LAG IN YOUR PLANS PLANS AND ACTIONS, AND BY COMMUNICATING IT TO THOSE WHO FOLLOW YOUR LEADERSHIP.

OUR CHALLENGE, TOGETHER, IS TO PROVIDE ENOUGH SUPPORT FOR SUCH BASIC RESEARCH FROM A VARIETY OF SOURCES, AND TO GUIDE IT WISELY TO PRODUCE THE MAXIMUM BENEFIT IN THE SHORTEST REASONABLE TIME. THAT GUIDANCE SHOULD BE GENTLE AND WISE, LIKE THAT OF A PARENT WITH A SMALL CHILD. FOR WE CAN PLANT AND TEND THE "SEED" OF RESEARCH, BUT WE MUST BE CAREFUL NOT TO PULL THE PLANT UP EVERY HOUR TO SEE HOW IT IS DOING, OR WE WILL NEVER BE ABLE TO HARVEST ORANGES FROM THE GROVE.

THE FIRST AND BEST GUIDE FOR RESEARCH IS A CLEAR, COMMUNICATED VISION OF THE GOAL AND THE NATURE OF THE PROCESS THAT IS UNDERSTOOD BY THE INDIVIDUALS

DIRECTING AND INVOLVED IN THE RESEARCH. THERE HAVE BEEN SEVERAL FINE STUDIES OF THAT KIND - E.G., THE ONE ON MATERIALS. WE NEED TO GO FURTHER. THE OTA REPORT MENTIONS JAPANESE SUCCESS WITH MITI, STA, AND AIST. THAT KIND OF BOTTOMS-UP PARTICIPATIVE DEVELOPMENT OF THE VISION IS GOOD. IT IS EFFECTIVE AND IT LEAVES PARTICIPANTS WITH BETTER UNDERSTANDING OF THE GOALS AND DIRECTIONS. YOU SHOULD ENCOURAGE IT. BUT, YOU SHOULD CONTRIBUTE SUBSTANTIALLY TO IT; YOU SHOULD EITHER LEAD THE DEVELOPMENT OF SUCH A VISION OR DELEGATE THAT TASK TO SOMEONE YOU TRUST TO DO IT WELL AND OVERSEE THE RESULT. PERHAPS YOU WILL WANT TO DIRECT THE NSF WITH THE U.S. DEPARTMENT OF COMMERCE (OFFICE OF THE ASSISTANT SECRETARY, TECHNOLOGY, INNOVATION, AND PRODUCTIVITY) IN COLLABORATION WITH INDUSTRY TO DEVELOP MORE SUCH VISIONS.

FOCUSING YOUR ATTENTION AND SUPPORT ON BASIC RESEARCH IS CORRECT, FOR THE PARTICULAR PIECE OF THE TOTAL U.S. EFFORT THAT I CAN PROPERLY ADDRESS. YOU CAN EXPECT INDUSTRY TO TAKE THE RESULTS AND DEVELOP THE PRODUCTS, PROCESSES, SERVICES, AND INFORMATION THAT WILL FLOW THROUGH THE MAINSTREAM OF THE ECONOMY. IT IS IMPORTANT, HOWEVER, TO ENCOURAGE AND STIMULATE

COUPLING OF THE RESEARCH WITH THAT DEVELOPMENT BY INDUSTRY; WITHIN THE SCOPE OF MY EXPERIENCE, MOST SUCH DEVELOPMENT THAT WAS GOVERNMENT FUNDED WAS NOT PARTICULARLY COST EFFECTIVE; THAT WASN'T BECAUSE OF THE FAILURE OF ANY INDIVIDUAL, IT WAS BECAUSE WE ASSIGNED THE DEVELOPMENT AND EVALUATION TASK TO THE WRONG PART OF THE U.S. SOCIETY.

SEVERAL CHARACTERISTICS OF INDIVIDUAL RESEARCH EFFORTS ARE IMPORTANT ENOUGH TO DESERVE A MOMENT OF YOUR TIME.

1. RESEARCH AND CREATION OF NEW IDEAS IS A PAINFULLY SLOW PROCESS AND WE MUST HAVE PATIENCE WITH IT. FORTUNATELY, IT IS A SMALL ENOUGH PART OF THE WHOLE THAT WE CAN TOLERATE THAT APPARENT INEFFICIENCY.

2. IDEAS AND PARTICULARLY TECHNOLOGY ARE EMBODIED AND TRANSFERRED LARGELY IN CAPABLE AND MOTIVATED PEOPLE. THE LIMITING RESOURCE IN RESEARCH IS EFFECTIVE, SELF-DIRECTED, SELF-DISCIPLINED PROFESSIONALS WHO ARE LIKELY TO CONTINUE TO WORK IN THE FIELD IN THE U.S. AND CONTRIBUTE TO FURTHER DEVELOPMENT OF THE KNOWLEDGE BASE OR ITS

APPLICATION IN THE U.S. GOOD RESEARCH PROGRAMS WILL INCREASE AND MOTIVATE AN INCREASING POPULATION OF SUCH INDIVIDUALS.

3. FOR THE U.S., IT IS BEST IF THOSE INDIVIDUALS ARE LIKELY TO STAY IN THE COUNTRY, TO CONTINUE WORKING IN THEIR FIELD, OR TO BE THE VESSEL IN WHICH THE TECHNOLOGY IS TRANSFERRED INTO ECONOMIC USE. YOU COULD AND SHOULD USE AS ONE MEASURE OF A PROGRAM THE PLANS OF THE PRINCIPAL INVESTIGATORS, GRADUATE STUDENTS, AND POST-DOCTORAL STAFF INVOLVED TO CONTINUE WORKING IN THE FIELD OF RESEARCH OR ITS TRANSFER WITHIN THE U.S. RETROSPECTIVELY, YOU COULD CALIBRATE THOSE ESTIMATES AND MEASURE SUCCESS BY THE ACTUAL CAREER EMPLOYMENT.

4. RESEARCHERS ARE INDIVIDUALS. THEY RESPOND TO THE BOUNDARY CONDITIONS WE IMPOSE ON THEM. THOSE AT UNIVERSITIES ARE PRESSURED TO BRING IN RESEARCH CONTRACT INCOME AND TO PUBLISH. YOU SHOULD ADD ATTENTION TO THE USEFULNESS AND THE USE OF THE RESULTS OF THEIR RESEARCH TO U.S. INDUSTRY AS A MAJOR CONSIDERATION IN FUNDING DECISIONS.

5. Modest size research teams are most effective. Individuals are isolated and large teams are unwieldy. A team of about five seems most effective. You might ask that that criterion be used as a general guide in evaluating research projects.

6. Research needs the benefit of the stimulation of interactions:

   A. with other quite diverse disciplines. That is not surprising, for most new ideas are a chance combination of ideas from diverse backgrounds; those in the same field have similar backgrounds and less likelihood of bringing different perspective and ideas to the combination. Unfortunately, one finds that that seems unnatural and rare.

   B. with the ultimate users in industry. That is best if it's at the working research and application levels.

You might use as measures of the likelihood of the creation and use of new knowledge, the number

AND NATURE OF SUCH INTERDISCIPLINARY AND RESEARCH-INDUSTRY INTERACTIONS.

7. THE NSF ENGINEERING RESEARCH CENTERS ARE GOOD. THEY HAVE ADEQUATE CRITICAL MASS. THEY INVOLVE INTERACTION WITH INDUSTRY; YOU SHOULD SUGGEST THAT BE STRENGTHENED.

8. EFFECTIVE RESEARCH TEAMS HAVE A LONG ENOUGH BUT LIMITED LIFETIME. A SENSE OF URGENCY ARISING FROM A SUNSET IS PRODUCTIVE. THE SUBSEQUENT REALLOCATION OF THE TALENTS AND RESOURCES CAN BE BENEFICIAL, THOUGH INITIALLY TRAUMATIC. FIVE YEARS MIGHT BE DESIRABLE; AFTER MORE THAN TEN YEARS, THEY MAY STAGNATE.

9. RESEARCH PROGRAMS ARE MOST EFFECTIVE IF CARRIED OUT AT A RATE INDEPENDENT OF THE UPS AND DOWNS OF THE ECONOMY. HOWEVER, THEY SHOULD NOT BE FUNDED IN PERPETUITY, BECAUSE THEY HAVE A FINITE EFFECTIVE LIFE. CONGRESS COULD ALLOW COMPANIES TO ACCRUE FOR RESEARCH AND THE DEVELOPMENT OF THE RESULTS INTO PRODUCT, PROCESSES, AND SERVICES, SO THAT THEY COULD CARRY OUT PROGRAMS AT THE BEST PACE, IRRESPECTIVE OF YEAR-TO-YEAR BUSINESS

CHANGES. COMPANIES HAVE TRIED TO DO THIS; SOME HELP WOULD BE BENEFICIAL. THE SEC AND THE FASB MIGHT ALSO ADJUST THEIR REGULATIONS AND STANDARDS.

THE U.S., THROUGH THE GOVERNMENT OR THROUGH INDUSTRY, IS SUPPORTING BASIC RESEARCH. IT IS ONLY FAIR THAT IT GAIN THE PRIMARY BENEFIT. IN FACT, IF THERE IS NOT SUCH BENEFIT FROM THE SUPPORT, THE SUPPORT WILL DRY UP. YET, THE BOUNDARIES ON THE COUNTRY, AS ON COMPANIES, ARE RELATIVELY POROUS. IN FACT THERE IS BENEFIT TO INTERCHANGE ACROSS THOSE BOUNDARIES. YET, THAT INTERCHANGE MUST BE TO MUTUAL BENEFIT. WE CAN HELP THE U.S. GAIN BENEFIT AND COMPETITIVE ADVANTAGE BY FOCUSING ON THIS PRINCIPLE AND BY GIVING PRIORITY TO THOSE PROGRAMS WHERE THE RESULTS ARE JUDGED MOST LIKELY TO FIT THE U.S. STRATEGY NEEDS.

AS YOU SEEK TO SELECT MEASURES FOR OVERSIGHT AND CONTROL, TWO ENGINEERING PRINCIPLES MIGHT BE USEFUL. FIRST, YOU CAN'T MEASURE SOMETHING BEFORE IT HAS OCCURRED. YET, YOU SEEK TO MEASURE RESULTS EARLY ENOUGH IN THE PROCESS TO BE ABLE TO EFFECTIVELY CONTROL IT AND TO CONTROL IT WITH MINIMUM LOSS OR OVERSHOOT. FOR INSTANCE, THE ULTIMATE MEASURE OF THE

EFFECTIVENESS OF U.S. RESEARCH IS THE IMPROVEMENT IN THE ECONOMY, IN INTERNATIONAL COMPETITIVENESS, IN THE NUMBER OF JOBS, AND IN THE WELFARE OF THE PEOPLE. HOWEVER, THOSE SIGNALS INVOLVE TOO MANY OTHER FACTORS AND COME TOO LATE TO BE USEFUL FOR CONTROL, ALTHOUGH THEY MAY BE IN A GLOBAL SENSE, REASONABLE RETROSPECTIVE MEASURES. HOWEVER, THERE ARE SOME OTHER MEASURES YOU MIGHT USE WHICH ARE MORE CLOSELY COUPLED AND CAN BE QUANTIFIED AND AGGLOMERATED, ALTHOUGH THEY ARE NOT EXPRESSIBLE IN DOLLARS. YOU MIGHT REASONABLY EXPECT NSF, DEPARTMENT OF COMMERCE, OR OTHER AGENCIES TO ACCUMULATE AND SUMMARIZE AND INTERPRET SUCH DATA FOR YOU ON A REGULAR BASIS. THEY SHOULD ALSO FEED THEM BACK TO THOSE CONDUCTING THE RESEARCH TO PROVIDE THE DESIRED GUIDANCE.

SECOND, THE HEIZENBERG PRINCIPLE, THAT ANY MEASUREMENT OF A SYSTEM WILL INFLUENCE IT, APPLIES TO SOCIAL SYSTEMS, LIKE COUNTRIES, AS WELL AS TO PHYSICAL SYSTEMS. IN FACT, IT IS EVEN TRUER OF THE ECONOMIC AND SOCIAL SYSTEMS. THE KEY IS TO SELECT MEASURES WHICH WILL INFLUENCE THE SYSTEM, POSITIVELY RATHER THAN NEGATIVELY.

SOME OF THE MEASURES YOU MIGHT USE FOR INDIVIDUAL PROGRAMS, SUGGESTED BY THE ABOVE OBSERVATIONS AND PRINCIPLES INCLUDE:

1. THE SCOPE, CLARITY, SPECIFICITY, QUALITY AND INDUSTRY ENDORSEMENT OF THE "BOTTOMS-UP GOALS" THAT NSF DEVELOPS. THOSE RESEARCH GOALS SHOULD HAVE A FIVE-TO-TEN YEAR SCOPE AND SHOULD BE RELATED TO NEEDS AND APPLICATIONS TEN-TO-TWENTY YEARS IN THE FUTURE.

2. WHETHER THE PRINCIPAL INVESTIGATORS, GRADUATE STUDENTS, AND POST-DOCTORAL FELLOWS INVOLVED IN THE PROGRAM DEVOTE FULL TIME TO IT AND HAVE CAREER PLANS INVOLVING SUBSTANTIAL EXTENSION AND USE OF THOSE RESEARCH RESULTS.

3. WHETHER THE TEAM IS OF AN EFFECTIVE SIZE, APPROXIMATELY FIVE PROFESSIONALS.

4. THE EXTENT AND QUALITY OF THE INTERACTIONS WITH INDUSTRY AND COMPLETELY DIFFERENT DISCIPLINES.

5. THE RELATED EXPENDITURES BY INDUSTRY ON RESEARCH IN ITS OWN LABORATORIES AND THE JOB OFFERS IT MAKES.

6. THE EQUALITY IN NUMBERS OF GRADUATE STUDENTS FROM A FOREIGN COUNTRY DOING RESEARCH ON A TOPIC IN THE U.S. AND THE NUMBER OF U.S. STUDENTS DOING SIMILAR WORK IN THAT COUNTRY.

SUCH MEASURES CAN BE USED AT TWO LEVELS. FIRST AND "EARLY-ON" THEY CAN BE IN TERMS OF EXPECTATIONS AND PLANS. SOMETIME LATER THE ACTUAL EXPERIENCE CAN BE MEASURED. FOR INSTANCE, ONE MIGHT ASK WHAT GRADUATE STUDENTS HAVE AS PLANS, AND WHETHER COMPANIES INTEND TO HIRE STUDENTS WHO HAVE DONE WORK ON A PARTICULAR TOPIC. LATER ONE COULD ASK WHETHER THE COMPANIES HAD FUNDED RESEARCH IN THEIR OWN LABORATORIES AND MADE JOB OFFERS, AND WHETHER THE STUDENTS HAD TAKEN EMPLOYMENT RELATED TO THEIR RESEARCH.

SEVERAL OTHER POSSIBLE ACTIONS WARRANT YOUR CONSIDERATION. FOR INSTANCE:

1. YOU SHOULD INCORPORATE IN YOUR APPROPRIATIONS GUIDELINES THAT FAVOR FASTER DIFFUSION OF RESULTS TO U.S. INDUSTRY AND THAT SLOW THEIR FLOW OVERSEAS.

2. YOU COULD SHOW, THROUGH LEGISLATION AND STATEMENTS, APPROVAL OF COLLABORATION ON BASIC RESEARCH BY COMPETITORS. THAT IS ALREADY BEING DONE TO SOME EXTENT. THE REINFORCEMENT OF IT BY STATEMENTS AND LEGISLATURE WOULD BE GOOD.

3. YOU COULD EXPAND THE PATENT PRINCIPLE TO ENCOMPASS RESEARCH AND ITS COMMERCIAL IMPLEMENTATION. THAT COULD GRANT EXCLUSIVE RIGHT FOR SOME LIMITED PERIOD TO A COMPANY THAT TRANSLATED RESEARCH FINDINGS, WHETHER PATENTABLE OR NOT, INTO A NEW PRODUCT/PROCESS OR BUSINESS. COMPANIES MIGHT COMPETE FOR THAT RIGHT IN TERMS OF THE SCOPE AND PROMPTNESS OF THE COMMERCIALIZATION AND THE SHORTNESS OF THE EXCLUSIVE PERIOD REQUESTED. WHERE THE SCOPES DID NOT OVERLAP, MULTIPLE GRANTS WOULD BE SENSIBLE.

4. YOU SHOULD REINFORCE THE CURRENT TREND TOWARD ALLOWING THE RESULTS OF GOVERNMENT-FUNDED RESEARCH, WHETHER PATENTS OR RESEARCH FINDINGS, TO BE GRANTED EXCLUSIVELY FOR SOME LIMITED TIME TO THE COMPANIES THAT PUT THEM PROMPTLY INTO USE.

5. Congress might grant a tax credit to industry for funding of basic research at universities, when it funds an equivalent amount of work in its own labs and establishes close coupling of the two efforts. Such credit might be limited to 1% of the company's research expenditures. That contribution to our deficit is suggested with care. It is relatively small. Like most parts of that problem, it could be handled in a different way, or offset by reduction in direct federal expenditure; actually, the nation would get more research done for the same cost to the government and it would be better coupled and used.

6. You might improve the laws so they better treat knowledge as a proprietary asset, at least for a period of about five years. With the rapid development and application of practical expert systems, which embody a great deal of proprietary knowledge, that is becoming an increasingly important need and opportunity.

7. Finally, you might use I.R.I., which was founded in 1938 under the auspices of the National

Research Council and whose members do about 80% of the industrially funded R&D in their labs to help. Specifically, you might ask them to:

A. Contribute to defining the research areas of greatest benefit to the U.S. and defining opportunities and goals for specific areas within that broad plan.

B. To stimulate and confirm direct working level coupling between industry and the researchers.

C. To assist, through their emeriti, in your evaluations and oversight.

D. To help involve professional societies in setting research goals and in the coupling process. I know, for example, that the Society of Automotive Engineers Research Executive Board is interested and willing to help.

I appreciate the opportunity you have given me to express my views and the interest you have shown. I

HOPE THE SUMMARY OF EXPERIENCE AND THE SUGGESTIONS WILL BE USEFUL TO YOU AND THE STAFF. I WILL TRY TO ANSWER ANY QUESTIONS YOU HAVE OR TO EXPLAIN OR EXPAND ON ANY POINT OF PARTICULAR INTEREST.

THANK YOU.

LE:CER - 4/28/86

DIRECTED BASIC RESEARCH: ITS ROLE AND CONDUCT

The value and continued support of basic research depends on how well it supports the primary strategic thrusts set for the company by top management.

Properly selected and executed Directed Basic Research (DBR) gives a company the earliest knowledge of, and opportunity to minimize, the uncertainty introduced by advances in basic science; it gives the greatest time to adjust to them and minimize disruptions and losses; it provides an opportunity to create a strong and favorable competitive position. The key is selecting the right fields of research, guiding the research, communicating the varying knowledge outputs, and translating the qualitative benefits of basic research into major company benefits.

Just as every company and every management is different, so does the amount and kind of DBR vary greatly from company to company. Also, companies, managements, business conditions, key decision makers, and, therefore, the correct portfolio of DBR change significantly with time. The research director must therefore know his company, business and management, as well as the relevant technology, and construct and continually adjust a portfolio of DBR that is suitably matched to them.

Research management has responded, and should, must and will respond, to the demand for more basic research. How well it selects, directs and interprets that basic research determines the benefits, extent and duration of the conduct and support of basic research. Selection and guidance of basic research is the most critical and difficult part of research management's job; the responsibility falls primarily on R&D directors, but the job must be done in collaboration with top management.

Research is an art. The management of research is even more of an art. It is creative; you can't do good art by the numbers. But, there are principles in research and research management, as there are in music and painting. Management of DBR involves problems and people with characteristics that differ markedly from most of those in industry. It requires having and communicating a sense of the long-term flow and cycles of technology, companies and industries, and making a suitable adjustment to them. Review and exchange of experiences improves managers' discharge of their major responsibility for "artistic" selection, direction, and utilization of *basic research*.

DBR Defined

Long-range, undirected, fundamental research is a search for new knowledge, with little consideration of its ultimate significance and impact. It has a very long, 10 to 50 years, time horizon. Some of it will eventually have utility to industry. Its objectives are to add to "truth and knowledge" and to train people. It is best left to the universities.

DBR, in contrast, is original scientific or technical work that advances knowledge in relevant (to corporate business strategies) scientific and engineering fields, or that creates useful concepts that can be subsequently developed into commercial materials, processes, or products and, thus, make a contribution to the company's profitability some time in the foreseeable future. It may not respond directly to a specific problem or need, but it is selected and directed into those fields where advances will have a major impact on the company's future core businesses.

Examples of DBR include particle science and technology, surface chemistry and physics, tribology, measurement and observation techniques, solid-fluid-liquid separation, adsorption at surfaces, artificial intelligence, energy beam modification of material properties, genetic microbiological "engineering," pattern recognition and interpretation, electronic material structure, and property manipulation and prediction. Such basic research is funded by a corporation (not the businesses or divisions) because it has broad and/or long-range applicability to various of the corporation's present or prospective businesses.

DBR is best conducted in industry, whose function is to create wealth. Such DBR probably has a two or three-year "lease" (but that needs to be five-to-eight years) and involves conscious access to the "truth-seekers" at the universities; it is beneficial if the people "circulate" between industry and universities. The major business benefits occur 5-to-25 years after the work is started but there are intermediate benefits, impacts on major strategic actions and capital investment decisions in the next 3-to-10 years, and measures of quality, value and progress.

Support of such DBR needs to be relatively independent of the business cycle. But funding can be kept tight. Initiation or pursuit of a line of research is not necessarily a "forever" commitment. Lines, not fields, of inquiry should be reviewed periodically, and those that do not prove productive or relevant to the business terminated so the support can be directed elsewhere. The termination rate should be known and significant—10-to-25 percent per year, for example.

This article is based on a report by a subcommittee of the Industrial Research Institute's Research-on-Research Committee. Chaired by Lamont Eltinge, director of research for Eaton Corporation, the subcommittee consisted of: Richard Bauman, director, Tire Research, The BF Goodrich Company; Richard Foster, principal, McKinsey & Co.; Kenneth Jarmolow, corporate director, Research and Development, Martin Marietta Corp.; A. Fred Kerst, vice president, Research and Development, Calgon Corp.; James McDonough, manager, Technical Development Division, Polysar Ltd.; James McGeer, director of research, Reduction & Raw Materials, Alcan International Ltd.; Robert Wismer, director, Technical Center, Deere & Co.

DBR should be selected that is coincident with the *primary* long-term themes of a corporation's purpose or strategic plan. It must interact effectively with those strategic visions and plans, and match their timing and time horizon. The research director is largely responsible for that interaction; some are "co-owners" of the long-range business plan.

Direction and Manning

To be effective and worth its cost, DBR must be correctly manned and managed. The management involves the right amount of direction and control, and the right amount of flexibility and freedom.

The conductor of DBR in industry is a "truth-seeker" in a carefully selected field; in addition, he should be exposed to "daily bread" problems and contribute both a scientific approach to their resolution and specific "new truths."

At the start, the principal investigator and the research director should have a good idea of what might come of the research; this will provide direction and necessary discipline. Research likely to significantly impact major strategic thrusts or major capital investment warrants the greatest weighting in selection. Positive and productive results are sought, but "negative" results showing valid limitations are valuable; their early establishment and clear interpretation permit redirection of people and money and avoidance of erroneous investments.

The assignment takes place at the interface between new science, technology and markets. Such research yields new information or knowledge for the innovator; consequently, attention must be given to disseminating it to innovators and to arranging the circumstances, including such information, that causes innovation.

The principal scientist carrying out such work needs freedom in work and travel. It should be taken for granted that he or she conducts first-class science, including literature searches, critical design and review of experiments, dialog on interpretation of the findings, consideration of what might come of his and his peers' work, and appropriate reporting. While it is necessary that he devote a substantial fraction of his time to his own research, that is far from sufficient. He also serves as a "gatekeeper" who applies not only the fruits of his work, but the fruits of the broader advances to which his work makes him privy and uniquely capable of evaluating and interpreting. His contributions to the field provide entree and a basis for evaluation. His activities should include "working" the appropriate scientific society meetings to find and mentor those nuggets of new science.

DBR is a vital activity that is difficult to evaluate and manage. The highest standards in the selection, maintenance and direction of those conducting such research are crucial; such assignments are not "soft"; they must not be a dumping ground for personnel problems. Rather, DBR warrants the assignment of personnel with the very highest level of scientific capability and disciplined self-direction.

Responsibility and Evaluation

The responsibility for DBR is primarily the research director's. But, that DBR must be coincident with the strategic thinking of the chief executive officer and the three-to-five other top executives who, in various ways, determine the company's direction. The research director must identify those few individuals and their basic strategy—formalized and verbalized or not. He must also inject into their thinking and strategic planning a recognition of DBR's significant impacts of technical advances on the corporation and the research requirements and effects of their strategic options.

It is unlikely and presumptive to expect that the technological content will be detailed in the minds of the CEO and key top executives. The research director must interpret emerging technical needs and responses from the nuances in the thinking of the CEO, key executives, the Board of Directors, others throughout the organization, and the next CEO. Selection and direction of DBR must be tailored to the organization's culture and their own level of patience, acceptance of risk, etc. The research director must estimate the amount of resources required to carry the product to commercial fruition and satisfy himself that enough resources are likely to be available to let the company "draw the last card"; otherwise, investment in the particular DBR will result in waste, frustration and destruction of general support for DBR.

The research director is the arbiter of how much and which basic research is supported. The basic research component of the R&D effort is a small fraction, often 10 percent or less, of the total R&D effort; thus, it is best evaluated and guided by the research director. He is best positioned to understand basic research, appreciate the general and specific contributions it makes to development programs, and decide about its funding.

If basic research is not being done, it is largely because the research director, the person who allocates effort and support to different facets of R&D, has not chosen to allocate it to basic research. He is the focus for the complaint that we haven't allocated enough support to basic research. He must answer the questions: "Should we do more? How do we do it well? How do we make basic research expenditures most effective? How do we capture enough benefits so that it is truly the right decision?"

Interaction with outside basic research is the best basis for evaluating in-house research capability, as well as being a stimulant and resource for it. The research director's personal oversight of staff interactions about scientific advances with outside experts in the targeted disciplines is the best means of evaluating the quality of the staff's science; other information indicates appropriateness of the selection [illegible] field and effectiveness of transfer and ex[illegible]ation of expertise.

Directing, using, and evaluating such work is more of an art than a science. Evaluation is qualitative, must deal with an inherently high degree of uncertainty, and is not amenable to discounted cash flow analysis. Evaluation must be based on broad experience showing that it is effective, and on management judgment that particular work is being carried out and utilized well. One measure of its effectiveness is the series of development projects it spawns. Another is the long-range technological contribution to major strategic and investment decisions; the research director is central to that contribution.

Advisory, audit, or review boards of experienced outside technologists can contribute significantly to the direction and acceptance of basic research programs. They can help with evaluations, specific leads, and reviews. Their effectiveness depends on an organization finding the correct members and on a clear and correct mission statement of their assignment and their adherence to it. Such boards and their members take several years "to get up to speed" on the company and its research; their contributions are likely to occur two-to-five years or more after initiation. "Sunset" for members, and even the board, should be established at the outset.

The boards are advisors or auditors; they do not devote enough time to the task to manage it. They can stimulate, contribute, and bring a detached but expert view to the R&D and basic research problems, and they can be of significant assistance to the company and the R&D director. But, the responsibility and authority are the R&D director's; it's his job.

Communications

The chief R&D executives are the most effective articulators of the corporation's expectations for the results of its basic research and the appropriate translators of the top management's broad concept of qualitative results expected from R&D investments and technical goals; their articulation and interpretation should reflect dialog with the chief executive officer. In very diverse corporations, the dialog of the chief research executive may be with the leaders of the major business units, rather than, or in addition to, the chairman and president of the company. In any case, it needs to be with top managers responsible for long-range planning and the execution of such plans.

The chief R&D executive should not be seen as an advocate of research but as an objective interpreter of what research can do for the company. It isn't effective for him to try too hard to quantify and sell the results of basic research.

A short (five-page executive summary, plus back-up information) review of the technological vector of the business by the research director, updated about every two years, can be a significant contribution to a company's strategic direction. It helps management appreciate technology and respond to it. It should identify the two or three most important technological issues. It should address the status of development of answers on those issues and important (to the business) technological developments; it should also address new technological issues and developments that are emerging.

A technology review meeting by top corporate management, based on the documents, is particularly effective; it is best if it is requested and/or endorsed by the CEO. The results of the meeting should be reflected in the management thinking summarized in the strategic planning documents; these should particularly emphasize technological futures, trends, and factors that will have mature impact on the business 5, 10, and even 20 years hence.

Feedback Invited

This overview of directed basic research is based on the experience and insight of the subcommittee members. It needs to be extended with specific examples and quantitative information. If readers are interested in supplying such information and insights, the Research-on-Research Committee will organize a suitable study and subcommittee. Please make your interest known to [illegible], Industrial Research Institute, Headquarters, 100 Park Ave., Suite 2200, NY, NY 10017.

Corporate Relationships

There is a need for top management attention to doing R&D well. The chief executive officer and a few other key top managers must have some concept of why they want to have R&D and what they expect it to produce. Such ideas are expected to be in the broadest terms, and are not likely to be quantified.

The role of top management in the exploitation of R&D as in most other missions, is to cause it to happen. They set the tone of the corporate culture and the emphasis on long-range technology-based projects. They are the only ones who can do so. The research director needs to interact with the CEO often enough, effectively enough, and openly enough to assure that the R&D element is included in the CEO's overall direction of the business and to understand the tone that the CEO sets for the business and assure that the research carried out is consistent with it.

There is a need to sell DBR to chief executive officers and other top managers whose early career experience in the 1950s led them to a jaundiced view of research based on some of the "ivory tower" thrusts at that time. Their understanding of the long-range, subtle, but vital, contribution that such in-house DBR can make, and the necessity of it for long-range survival, needs to be developed. Yes, many corporate CEO's and top managers do not have the time or background to establish, personally, that the basic research they are funding is good. Their support is largely based on their relationship with their chief R&D executive; they follow his lead on the small fraction of R&D support directed to basic research. ☐

DISCUSSION

Mr. Fuqua. Thank you. We will make your printed testimony a part of the record.

You were talking about tax reform. Should there be a tax or a royalty levied on the users of knowledge derived from Government-funded research, and then the proceeds of that to support or be dedicated to the furtherance of basic research?

Dr. Eltinge. I think the answer is yes, and there is an interesting example of that not involving Government money—but I believe that Monsanto has a major commitment to medical research at Washington University in St. Louis, and they have agreed that, in spite of the fact that they paid for it in the first place, they will pay royalties on it. But the quirk is that the royalties they pay will go back to funding some more research in the same field, and that's great. First of all, you're betting on a winner. Second, you induce close coupling.

And they had one other thing that was interesting there, that that research is guided by a joint, equal partner number of people both from the university and from Monsanto. Both parties started out on that with a bit of concern that there would be bloc voting, that Monsanto would vote against the university. They have yet to have a bloc vote because obviously a bloc vote is tied and is a non-decision. So what they're finding is that everybody really brings his own best judgment to that and it's working well. It's a good program.

So I think the answer to your question is it is not unreasonable to expect——

Mr. Fuqua. But that's a voluntary program.

Dr. Eltinge. That's a voluntary program. The same principle could apply, where the Government which really is an agent for the U.S. citizen, who has paid the taxes in the first place, who has put up the money, to say, "I would like to get more leverage off of it. If you would like to use the results, fine. Pay for them and we'll plow the money right back in that same place and tell them to keep going."

One observation you might make on that, though, is that there seems to be a life-cycle of research efforts, and we need to support them fairly steadily and fairly consistently over a period of 5 or 10 years. But you will find that after a while, like a fruit orchard, it kind of goes dead. We all wish that wasn't the case, but I think it's true. Recognizing that would be an important part of wise stewardship of the resources that we have.

Mr. Fuqua. Since it has been the policy of the Federal Government to support basic research, is there a tendency in private industry to kind of "let George do it" and not be responsible for supporting their appropriate share? I'm speaking of industry.

Dr. Eltinge. Yes. I think that has been made as a criticism. It is perhaps fair. I think it's understandable.

One of the problems with basic research, remember, is that you do the basic research many years before the benefits accrue to the company. Furthermore, we have a long tradition, which may need to be modified a little bit, that says that when you create basic science results, they ought to be given away to everybody as a common good. Well, you know, and we have all learned since we were kids, that that which everybody owns, nobody owns, and doesn't get very good support. So I think in those cases companies are driven to invest too little in basic research. I think there are ways to change the relationships between the parts of our society just a little bit, so that you can encourage what is really starting already, and that's a trend for industry to put a little more money into basic research, most of which is done at the universities.

There are a few outstanding examples of industrial labs that really do basic research. I think most of the basic research has been done at the universities or in some of the Government laboratories. The key isn't where it's done; the key is that that basic research is closely coupled into U.S. industry, so that the transfer occurs as fast as it can and the use is there. That's where the key has to be. I think we can, in answer to your question, stimulate more industry funding of basic research. Some form of tax credit, not to fully pay for it, but to absorb part of the cost would, I think, enhance that trend, and I think it would be healthy, because you then would have a natural form of coupling.

Mr. FUQUA. How can we compare our research position with that of other countries, and are there indicators or signs that help us try to determine the areas that we need to put emphasis in?

Dr. ELTINGE. I'm not sure I'm really qualified to answer that. I can see bits and pieces, but I don't think I know the whole answer to it.

Obviously, the one thing that you see is the people from one place and another meet. They, on a 1-to-1 basis, know where the good work is being done. They see the progress. You almost have to get down and trace that.

I guess the measure I would use, if I was trying to do it, would be to find out where they speak with their feet and where they speak with their money. Dick was commenting on that; who is licensing. Usually the license is of a patent. It is not the result of basic research. The basic research comes before the patent. But the other one is who is sending graduate students where, or employees where. We find people from other countries come to this country to study as graduate students and to come as "postdocs." We do see some of the other. But you have to see who goes there and who really provides the motivation behind that, and then what they do when they come back—or, if they come back. Because it's in that form that you're really accumulating the knowledge.

You wouldn't send somebody to go do work in a foreign laboratory for 3 years, and then bring him back unless you thought there was something good there. That's where people really will tell you by their actions what's worthwhile.

Mr. FUQUA. You indicate that——

Dr. ELTINGE. And I don't have numbers from that kind of a study.

Mr. FUQUA. You indicated that you were speaking for yourself and not the Eaton Corp.——

Dr. ELTINGE. Yes.

Mr. FUQUA [continuing]. But does your corporation have any indicators that influences this decisionmaking process and where it should shift gears, and so forth, in their emphasis, particularly as it relates to research?

Dr. **ELTINGE**. As regards basic research?

Mr. **FUQUA**. Yes; and particularly for international markets.

Dr. **ELTINGE**. I don't think we have any that I would consider good, quantitative indicators. I think it is fair to characterize us as a step downstream from the basic research. We are increasingly trying to couple to the basic research being done at universities because we believe that's part of our being a healthy, growing, progressing company. But we don't have any good indicators. We simply do such things as try and count the number of schools.

We qualitatively say, "Well, we've got a research program being funded at that university, and we've got this project in our own labs that's related to it," but we have not had enough experience with that to then be able to interpret those in terms of trends. I think, though, we will see more of that and I think through things like the IRI you have a broader background against which to interpret.

Mr. **FUQUA**. As we approach the turn of the century, are there any adjustments or changes that you see in the present methods and procedures that must be considered and possibly implemented to make our funding future investments far more effective?

Dr. **ELTINGE**. You're speaking now in terms of funding through the Government?

Mr. **FUQUA**. Yes, yes.

Dr. **ELTINGE**. I think the procedures that I would use would really be to introduce in that decisionmaking process some of the measures that I have talked about. One is how closely is that university research coupled to industry, not in the sense of pulling the university research in to do development work, but in terms of making sure that there's enough industry interest, there's enough interaction, so that the research is guided.

A second measure is related to some good work by NSF and the National Science Board to try and identify some major areas. There was one study that one thinks of in the whole area of materials technology that laid out some principles. I think you might stimulate more of those through NSF, and through the Department of Commerce Office of Science Technology and Innovation headed by Bruce Merrifield. I would strongly encourage there that you ask the IRI, particularly through its Emeriti Activity Group, to help you perceive what those goals unique to the United States ought to be.

You know, it isn't a matter of us trying to say we're going to meet all world competition on all fronts head-on. You have to be somewhat selective. Those ought to be the things that deal with where the United States has particularly unique needs, and particularly strong resources.

One that comes to mind is we have a tremendous amount of coal, and as we go beyond the era of oil being the primary energy source in the world, maybe back to where coal is, we need some science that's really addressed to how we deal with coal in a far better way. That science could be stimulated. So you could set targets or areas like that and then use those to guide where you put a great deal of the Government funding, although again you need to guide it gently because it's a tender seed.

Mr. FUQUA. Thank you very much for joining us this morning.

Dr. ELTINGE. Thank you for your time. I have enjoyed being here.

Mr. FUQUA. We appreciate your contributions.

[Whereupon, at 12:09 p.m., the task force was adjourned to reconvene on Thursday, May 1, 1986, at 10 a.m.]

[Questions and answers for Dr. Eltinge follow:]

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25 July 1986

Congressman Don Fuqua
Chairman
U.S. House of Representatives
Committee on Science & Technology
Washington, DC 20515

Dear Congressman Fuqua:

Thank you for your letter of 17 June 1986. It transmitted four follow-up questions and requested answers that would supplement my testimony of April 30th before your Committee.

In connection with the point that attention be paid to the use and usefulness of government-funded research to U.S. industry, you asked how one would determine whether the research was germane, given the uncertainties involved in basic research. I believe I suggested in both my oral and my written testimony that concrete indications of interest by industry were a good, if not the sole, indicator we might use. Industrial research leaders deal with such uncertainty regularly. They must continually judge which research areas are promising and likely to yield useful results; that judgment is imperfect but good.

Good indicators, as well as positive factors in coupling the research to the industry that must translate it into economic vitality, include the following concrete and observable actions by US Industry:

. visits to campus to review the work,
. recruiting of the graduates who conducted the work,
. coauthorship of publications,
. referencing of papers or reports,
. partial funding of the research by industry
. letters indicating interest in using the expected results.

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Those vary in timeliness, but all tend to lag decisions to fund. Therefore, inclusion of evidence of industry viewpoint and interest in the evaluation process would be beneficial. The views of industry are most important as to the relevance of the field in which the research is done. The individual research investigator must have substantial latitude in selecting particular direction; in fact, that fresh viewpoint is a major benefit of independent basic research.

You also asked how priority on research funding could be established, given the five-to-ten year life cycle of such research. First, my mention of a five-to-ten year life cycle was to suggest that a particular line of inquiry might last that long, but should not go on forever. Actually, the lead time between basic research and commercial practice in many cases is more like ten-to-twenty years, with the time frame being longer for the more fundamental and important developments. Second, I cannot suggest a mechanistic formula for establishing priorities; rather, I would recommend combination of the kind of evidence of industry interest, expressed above, and an overall sense of those areas in which the nation's interests are best served, because the expected applications are consistent with those directions in which we have some natural advantage or national priority.

You asked what data I had in mind when suggesting NSF, Department of Commerce, National Academy of Engineering, or with the help of other agencies, like I.R.I., accumulate, summarize, and interpret data for the guidance of research. What I had in mind was the overview perspective of experienced research leaders on those areas that seemed to hold the most promise of significant advance and wherein significant advances would have substantial importance to the nation's economic health and international competitiveness. What is needed is information, knowledge and judgment, based on facts and experience, rather than raw data. An excellent example of the kind of overview I had in mind was the National Academy of Engineering Study on Advanced Materials. Such reports should be updated every three-to-five years, and reports for different fields should be prepared as appropriate. I believe there should be a substantial amount of support from the Department of Commerce, NSF, the National Academy of Engineering, and, perhaps, DOD and the House, Science and Technology Committee for such studies. Those should be coupled to a substantial amount of "volunteer" input from experienced industrial research leaders. This means of guiding the US "investment" in research is an example of "how priority on research funding could be established."

You mentioned my suggestion that we emphasize interactions between disciplines and then asked why attention was necessary, given the many meetings that are held. Although there are many meetings, their focus and attention are largely in specialized areas. The result is that they do not stimulate interdisciplinary interaction very much. Such interdisciplinary interaction is difficult because each discipline has its own mental set, jargon, and values; yet, the combination of ideas and insights that one seeks in basic research is from that diversity.

Participants in such interdisciplinary interaction must work hard to deal with those differences; it is easier for "birds of a feather to flock together." The point is that it is necessary and beneficial to consciously counteract that tendency in a number of ways. The NSF-Engineering Research Centers are a good step in that direction. The principle might even be broadened by including a few participants from non-engineering and, perhaps, even non-scientific disciplines in the projects at those Centers.
Expert Systems and other artificial intelligent ideas and approaches arising out of the interaction of cognitive psychologists and computer scientists are a worthwhile example of beneficial interdisciplinary interactions. While there is substantial chaff and confusion from such interaction, there are also a few very valuable nuggets that in time will be winnowed and used. The only concrete suggestions I can make for stimulating interdisciplinary interaction is that there be positive and measurable, although probably qualitative, allowance for it in the NSF and other government research decision making.

You pointed out that industrial sector proprietary restraints act against coupling between industry, government, and the universities, and that in the public sector, the information is open to all, and asked what must be done to achieve effective coupling.

The really effective coupling is one-to-one communication between those knowledgeable in the field - in this case, university researchers and industry researchers and users of the results of the research. Including evidence of such coupling as a criterion in your selection of programs or projects to fund is an important contribution to making such coupling occur. To the extent the focus is on the new science and new concepts that are developed, rather than on their applications, the problems with proprietary restraints can be minimized. The university researcher can be employed as a consultant on the applications; his consulting contract would handle the proprietary aspects. Such employment of university researchers is a concrete and positive indicator of coupling. Industry must translate the results of the university research into economic progress so it has the primary responsibility for achieving the gain from the coupling. We can't make such coupling happen, but we can remove barriers and provide encouragement. We can specifically allow limited exclusive access and rights to the results of federally funded research in exchange for putting them promptly into practice; that stimulates gain from the coupling.

Finally, there is one specific issue that Congress could address that should contribute substantially to the rate at which new discoveries are transformed into commercial products and processes. That is in the field of "Liability FOR UNKNOWN HAZARDS." We the people of the USA seek the benefits of technological advance; we must accept some of modicum risk. Legislation could limit the liability for risks incurred before they were generally recognized by the technical expert community; some provision could be made for alertness and review of early concerns to provide good judgment in the application of the principle. The current situation with asbestos is an instructive example.
Action in the drug field is another; even there, there is concern whether the balance between benefit and risk is optimal. I do not have a particular approach to recommend but believe that attention to liability could be helpful.

Sincerely,

Lamont Eltinge
L. Eltinge
Director of Research

LE:cer (26.41)

cc: Mr. Melvin Ross

RESEARCH FUNDING AS AN INVESTMENT

THURSDAY, MAY 1, 1986

House of Representatives,
Committee on Science and Technology,
Science Policy Task Force,
Washington, DC.

The task force met, pursuant to call, at 10 a.m., in room 2325, Rayburn House Office Building, Hon. Don Fuqua (chairman of the task force) presiding.

Mr. Fuqua. Today's hearing concludes the 3-day series by our science policy task force on funding as an investment.

We have received a basic overview on the subject from OTA and that statements of witnesses from academia, finance and industry.

Our first witness today will be Sir David Phillips, chairman of the advisory board for the research councils of the United Kingdom. We look to Sir David for some comments on the contemporary picture of research policy in Great Britain.

Our second witness will be Dr. Rustum Roy and Ms. Deborah Shapley, who coauthored a book which presents a new interpretation of the 1945 Bush report, as seen in 1985.

Dr. Mary Mogee, a former member of OTA and of the Patent Office, will provide information on patents and patent policy.

Our concluding witness will be Dr. Richard Hallion, the historian for the U.S. Air Force, will speak about basic research conducted by the Government as far back as 70 years ago and its contribution to the fields of aeronautics and space.

Sir David, we would be pleased to hear from you at this time. We are always delighted to have you in our country and also to hear the wisdom of our friends from Great Britain.

[A biographical sketch of Sir David Phillips follow:]

Professor Sir David Phillips, F.R.S.

Name: David Chilton Phillips.

Occupation: Professor of Molecular Biophysics, University of Oxford.

Date and place of birth: 7th March, 1924: Ellesmere, Shropshire.

Home address: 3, Fairlawn End, Upper Wolvercote, Oxford OX2 8AR.

Business address: Laboratory of Molecular Biophysics, Rex Richards Building, South Parks Road, Oxford OX1 3QU.

Marital status: Married 1960 to Diana Kathleen (née Hutchinson): 1 daughter.

Education: 1935–42: Oswestry Boys' High School, Shropshire; 1942–44: University College, Cardiff and (1948 1st Class Hons. B.Sc; 1947–51: 1951 Ph.D. in Physics).

Appointments: 1944–47: R.N.V.R. Officer. 1951–53: Post-doctoral Fellow, National Research Laboratories, Ottawa, Canada. 1953–55: Research Officer, National Research Laboratories, Ottawa. 1956–66: Research Worker, Davy-Faraday Laboratory, The Royal Institution, London. 1960–66: Member of the External Staff of the Medical Research Council. 1966: Professor of Molecular Biophysics, University of Oxford, and Professorial Fellow of Corpus Christi College, Oxford. 1982: Nonexecutive Director of Celltech Ltd.

Public Service: 1983: Chairman of the Advisory Board for the Research Councils. 1985: Member of the Awards Assembly of the General Motors Cancer Research Foundation. 1981: Member of Board of Governors, Weizmann Institute. 1980: Member, Advisory Committee on Harvard-Monsanto collaboration. 1973: Member, Executive Council of Ciba Foundation. 1976–83: Biological Secretary and Vice-President of the Royal Society. 1978–83: Royal Society Assessor on the Medical Research Council. 1974–78: Member of the Medical Research Council. 1974–76: Founder Chairman of the Cell Biology and Disorders Board, Medical Research Council. 1981–85: Member, Scientific Advisory Committee, Biozentrum Basel. 1979–85: Fullerian (visiting) Professor of Physiology, Royal Institution, London. 1976–85 Member, Council of Society for the Protection of Science and Learning. 1982–84: Founder President, British Crystallographic Association. 1976–84: Chairman, Visiting Advisory Committee, Max-Planck Institute of Biochemistry, Martinsried, F.D.R. 1975–81: Member, Council of International Union of Pure & Applied Biophysics (IUPAB). 1971–77: Member, Council of European Molecular Biology Organization (EMBO). 1972–75: Chairman, Crystallography Group of the Institute of Physics. 1969–75: Member, Science Consultative Group of the B.B.C. 1972–73: Vice-President of the Royal Society. 1971–73; 1969–70: Member, Council of the Royal Society. 1968–72: Member, Committee of the Biochemical Society. 1969: Chairman, First Harden Conference of the Biochemical Society. 1968–71: Member, Committee of Managers of the Royal Institution.

Membership of Editorial Boards etc: 1984: British Association for the Advancement of Science, "Link-Up". 1984: I.R.L. Press Scientific Advisory Board. 1983: *Biochemistry*, Editorial Advisory Board. 1975: *Interdisciplinary Science Reviews*. 1973: *Handbook of Biochemistry & Molecular Biology* (C.R.C. Press). 1971: Editor (with F.M. Richards), Oxford University Press *Atlas of Molecular Biology*. 1971–81: Consultant Editor (with H.L. Kornberg): *International Review of Biochemistry* (Series I & II); Medical & Technical Publishing Co. 1965–77: Editor, *Journal of Molecular Biology*. 1975–76: *Frontiers of Biology*. 1973–76: *Journal of Theoretical Biology*.

Membership of Learned Societies: 1957: Fellow of the Institute of Physics. 1960: Member of the British Biophysical Society. 1965: Member of the Biochemical Society.

Lectures: 1984: Irvine Memorial Lecture, St. Andrews. 1983: W.H. Stein Memorial Lecture, Rockefeller University N.Y. 1982: Plenary Lecture: Royal Australian Chemical Institute, Convention, Canberra. 1980–81: The Christmas Lectures, The Royal Institution. 1979: A.L. Patterson Memorial Lecture, Fox Chase Cancer Center, Philadelphia. 1977: Plenary Lecture: 50th Anniversary, Netherlands Society for Biochemistry. 1976: Plenary Lecture: International Biochemistry Congress, Hamburg. 1972: Plenary Lecture: International Crystallography Congress, Kyoto. 1971: Harvey Lecture, New York. 1971: Krebs Lecture: Federation of European Biochemical Societies (FEBS) Meeting, Varna, Bulgaria. 1968: Hassel Lecture, Oslo, Norway. 1967: Plenary Lecture: International Biochemistry Congress, Tokyo. 1966: Almroth Wright Memorial Lecture, London.

Honours & Awards: 1985: Foreign Associate of the U.S. National Academy of Sciences. 1984: Hon. Fellow of Portsmouth Polytechnic. 1983: Hon. D. Univ., University of Essex. 1982: Hon. D.Sc., Exeter University. 1982: Hon. D.Sc., Warwick University. 1981: Fellow of University College, Cardiff. 1979: Knighted. 1979: Charles Leopold Meyer Prize of the French Academy of Sciences (jointly with D.M. Blow). 1978: Hon. D.Sc., University of Chicago. 1975: Royal Medal of the Royal Society. 1975: Hon. D.Sc., University of Wales. 1974: Hon. D.Sc., University of Leicester. 1971: CIBA Medal of the Biochemical Society. 1971: Krebs Medal of the Federation of European Biochemical Societies (FEBS). 1969: Hon. Member, American Society of Biological Chemists. 1968: Feldberg Prize. 1968: Foreign Non. Member, American Academy of Arts & Sciences. 1967: Fellow of the Royal Society. 1964: Member of the European Molecular Biology Organization. 1951: Post-Doctoral Fellowship, National Research Council of Canada. 1948: University of Wales Post-Graduate Studentship. 1942: Shropshire County University Scholarship. 1939: Shropshire County Major Scholarship (High School). 1935: Shropshire County Scholarship (High School).

STATEMENT OF SIR DAVID C. PHILLIPS, PROFESSOR OF MOLECULAR BIOPHYSICS, UNIVERSITY OF OXFORD; AND CHAIRMAN, ADVISORY BOARD FOR THE RESEARCH COUNCILS, UNITED KINGDOM

Sir DAVID PHILLIPS. Thank you very much, Mr. Fuqua. I am delighted to be here and to be able to observe the activities of your committee.

Should I perhaps begin by making a short statement about the overall scene in Britain as I see it and how it's evolved? And then I would be very glad to answer any questions you would like to put to me.

Mr. FUQUA. Thank you.

Sir DAVID PHILLIPS. I think the origins of research—publicly funded research—in Britain are a little different in emphasis from those in the United States in that the need for publicly funded research was first perceived rather dramatically in the middle of World War I when deficiencies in high technology and science based industry in the United Kingdom led to shortages in such materials as high-quality steel or fine chemicals and drugs, and so on. And this led in the middle of the First World War to the establishment of Government agencies—the first of which was the Medical Research Committee—but the more important perhaps in this context, the Department of Scientific and Industrial Research.

These institutions were set up quite consciously with the aim of benefiting the economy of the United Kingdom and of promoting the right sort of developments in industry. That was formalized in 1919 in the establishment of the Research Councils system which, in the early 1930's, embraced also the Agricultural Research Council, taking under the Government wing a fairly large number of institutions which had been funded privately earlier on, but were now seen to need public funds to continue.

In the interwar period, these bodies aimed very much at immediate practical problems, but increasingly they perceived, too, that there were many practical problems which could not be directly addressed, but which might be addressed if the science base, if the base were in some way increased. It is easiest, perhaps, to point to medical examples or however that might be so; cancer research, for example, could hardly proceed until there was a better understanding of cell biology.

So that in the period between the wars, in addition to addressing practical problems, the Research Councils increasingly funded research in universities of a much more basic kind. This led to a joint product, that is to say, research results were produced but also a fairly large number—growing number—of people experienced in research were produced. And that is the joint product of the system.

That was fairly clearly seen to pay off in World War II when many of the scientists, in particular, trained in this way, were in a sense directed into various branches of Government research—for example, in the development of radar, in the development of operations research, to pick two of them offhand as examples. So that by the end of the Second World War, at the time of the famous Bush report, the general ethos that publicly funded science led to important consequences of a social and economic kind, was fairly well-embedded in the political and scientific consciousness, and the climate was right for a considerable development in publicly-funded research through the Research Councils and in the universities. And that process led to fairly large annual growth rates certainly up to the mid-1960's, which involved both annual increases of sometimes as much as 10 or 15 percent in real terms in Research Council budgets and the creation of a fairly large number of new universities, all of which aimed to engage in scientific research at a fairly high level.

This system then led, by 1965, to a slight reform—at the end of which there were five Research Councils—the Science Research Council, Medical Research Council, Agricultural Research Council, Natural Environment Research Council, and Social Science Research Council, and some 50 universities, all of which were engaged in research.

Now here I think I should mention a very important difference between the system in the United Kingdom and the system in the United States, that is to say, the research in universities is supported by what we call the dual support system. These universities are all publicly funded in effect. The amount of private funds which go into the system is much the smaller part, probably less than 10 percent, so that they all receive public funds through a Government agency called the University Grants Committee. This committee allocates to the universities funds which are partly in support of research.

It is difficult to estimate exactly what proportion of these funds support research, but the budget this year allocated by the University Grants Committee to the universities as a whole amounts to about £1.4 billion. The estimates are that, of that sum, something like £600 million are in support of research, and that has to be apportioned partially to support the salaries of academic staff who are engaged in research; partly to maintain the buildings and facilities that are used for this research; partly and probably in rather small part to support the consumables and equipment that are needed for research.

On the other hand, the Research Councils distribute roughly the same sum of money—£600 million—to research both in their own institutes and in the universities. And the university support is provided mainly in the form of project grants of limited duration in response to proposals from university research workers to do particular pieces of research. It follows from the fact that a dual support system exists, that the research grants distributed by the Research Councils do not include an overheads element. The overheads are provided by the University Grants Committee part of the budget.

You could, of course, say that it also follows that one needs to have a certain amount of monitoring to make sure that all those people who should be doing the research supported rather generally by the University Grants Committee actually are engaged in research. Otherwise, one is in effect paying the overheads without actually having the work done.

Government policy, at the moment, is to maintain, enhance, and strengthen the science base, but at the same time, to do that within the context of constraining public expenditure. And that, of course, is a rather difficult balancing act to achieve. It leads inevitably to intense discussion of how one evaluates the results of research, to what extent it can and should be concentrated, to what extent priorities should be identified. And these, I am sure, are all concerns which we have in common with activities in the United States.

What has happened since 1981 is that the Research Council budgets have actually increased in real terms by some 6 percent between 1981 and 1986, so that the view that research funding has actually positively been cut in the United Kingdom over that period is, should I say, exaggerated.

Although in real terms the Research Council has increased by 6 percent, it has to be said that it has at the same time been responsible for additional expenditures, not all of which actually buys science. That is to say, some, for example, some restructuring of the system has taken place. The number of agricultural research institutes, for example, has been reduced. The number of staff permanently employed in agricultural and in environmental research has been reduced. These restructuring operations cost money that has come out of this 6-percent increase in the budget.

The work force in the Research Councils and universities is aging. There are additional superannuation costs which have to be carried. These costs have come out of the apparent increase.

There have been other marginal increases, like increases in the tax on building works, increases in national health, Social Security payments—all of these had to come out of the apparent increase. So that when you balance all of these things, what I think can reasonably be claimed is that the Research Council budgets have been held roughly constant over the period 1981 to 1986—in terms of the science that can be bought, especially if you call that the science of 1981.

An argument that we go into quite a lot is to what extent is science becoming more expensive in terms of more expensive consumables, more sophisticated equipment, and so on, and saying that the budget has been held constant, I am not paying any attention to those rather difficult problems. By contrast, the budgets of the universities since 1981 have not been held constant and this has had an effect on their ability to support research in their wing of the dual support system.

In 1981–82, the universities' budgets paid through the University Grants Committee were reduced by about 8 percent. Since 1981–82, there have been a number of earmarked additions to university budgets; for example, to provide for some increase in the provision of sophisticated equipment, and very selectively, to support some particular programs, to increase the number of people in science and engineering, to increase information technology, and to provide some new, young lecturers in critical subjects.

That, I am afraid I can't quantify. But over the period since 1982, there have been some increments to the budget to provide for those things. But now the estimate of the University Grants Committee is that in this year and over the next 2 years, the University Grants Committee budget is falling in real terms by about 2 percent per annum. And that is undoubtedly having an effect on the way in which universities are able to support their research programs; that is to say, the university side of the dual support system.

The question, then, of how large and how well provided the science base should be, if I, for the moment, define the science base as that supported by the universities and the Research Councils system, is something which the Government is advised about by the committee that I chair, the Advisory Board for the Research Councils. I hope that what I have said will illustrate that we have apparently persuaded Government that it should maintain, and if possible, enhance, the science base, but that it should do it in a rather more selective way than it did previously. That is the somewhat apparently unselective university element of the science base is under pressure; the Research Council-directed element of the science base has been rather better maintained.

I should add that the present activities of the University Grants Committee are aimed in a rather considerable exercise now in progress to increase the selectivity within the university system. At the moment, as I have said, all universities aim to be engaged in both teaching and research in roughly the same proportions in each institution. There is something of a move toward differentiating between institutions so that some become more specialized teaching institutions, teaching universities—perhaps even liberal arts colleges, in your terms—whereas others remain fully fledged research universities.

Finally, Mr. Chairman, if you would bear with me, in the general scene of Government support for research, the other major departments of Government also engage in research activities. Those that I have been describing that maintain the science base are the responsibility of the Department of Education and Science. But other departments, of course, particularly defense, trade and industry, energy, environment, health and social security, agriculture, nutrition and food, all have research budgets and support research. The tradition is that these departments have responsibility for their own programs and that responsibility is decentralized.

As against that, over the last 4 years, there has been published annually an annual review of Government R&D which sets out the expenditures of all these departments, alongside some account of the objectives of their program. And this is leading toward a general discussion of the overall Government program for supporting research and development and to public questioning of what are the purposes of this expenditure. That is some process which is now in evolution.

Finally, Mr. Chairman, may I say that having looked at some of the papers of your committee, I must thank you for the careful thought and analysis that has gone into them which is very helpful to our thinking. And I shall look forward very much to hearing reports of your activities which address problems which I believe are very similar to those in the United Kingdom, and I am sure in all Western countries.

Thank you.

DISCUSSION

Mr. **Fuqua**. Thank you very much, sir. It is very nice to hear what—I guess it is not nice, it is nice to have you here, but to hear the problems that other countries are having in financing science.

You indicated that you are achieving a 2-percent-per-annum reduction. How long do you anticipate that to continue and when did it begin?

Sir **David Phillips**. That, Mr. Chairman, is in the university side of the picture, and that is the estimate which I think is likely to be published by the University Grants Committee during the course of the next few months.

I think there was, as I have said, a drop in university support of about 8 percent—possibly a little more—announced in 1981–82. Since then, the funding has been eroded slightly by various factors that I mentioned, but also increased by ad hoc increments for particular purposes, leading up to the 1985–86 year; and from then onward for 3 years, the estimate is that there will be a 2-percent decline per annum.

Now, that, I have to stress, is a very serious decline. To put it another way—since we have 50 universities and they are all very largely publicly funded, a 2-percent decline means that one medium-sized university ought to go out of business if the others are going to keep on at the same sort of level of activity.

Mr. **Fuqua**. How long do you anticipate this 2-percent decline to continue, or is there plans for reversal of that——

Sir **David Phillips**. The Government's expenditure plans tend to run over a 3-year period. The present figures suggest that this decline will go on over the current 3-year period. I hope very much that that will be being reviewed by Government this year.

Mr. **Fuqua**. So you do not see any reversal of that trend within 3 years, but after that there may be——

Sir **David Phillips**. A leveling off.

Mr. **Fuqua** [continuing]. A leveling off.

Sir **David Phillips**. Yes.

And it may be that Government, over this year or next, will rethink the situation and stop that trend.

Mr. **Fuqua**. Do you have tax incentives for British corporations that could fund—you indicated that most all of it were publicly supported funding for research.

Are there any tax incentives for industrial contributions to basic research, or cooperative programs with universities where private funds and public funds are used to support basic research?

Sir **David Phillips**. Yes, there is considerable activity at the moment in trying to improve the interface between the publicly funded system and private industry. At the moment, I believe donations to universities are simply relieved with the normal corporation tax. There isn't any additional incentive. Though in the recent budget, provisions were made which do, I think, facilitate both private individual and private corporation contributions to universities and other charities, which we hope will have some effect. There is lively discussion of what might be done on that front at the moment, but no particular tax incentives that I can describe to you.

Mr. FUQUA. We had testimony before us last year concerning some of the science evaluation methods that have been used in Great Britain and that there was a form called the converging indicators that received a great deal of attention.

I wonder if you might share with us your own assessment of the strengths and limitations of their studies.

Sir DAVID PHILLIPS. You are referring there, I think, to the work of——

Mr. FUQUA. Professor Martin.

Sir DAVID PHILLIPS [continuing]. Irvine and Martin at the Science Policy Research Unit in Sussex, in which we do take a great interest.

My personal view is that their studies have been really quite valuable. Their studies, for example, of optical telescopes and astronomy, some of their recent studies of high energy particle physics at CERN have given, I believe, some valuable insights, but mainly of a retrospective kind. Where I see some need is for the development of methods—and this is asking a great deal—which would be of more prospective value.

If we can learn from history, I think we can learn from these particular studies. Learning from history isn't always that easy.

Mr. FUQUA. Could you comment on the comments made by Dr. Steven Locke, who is the editor of the British Medical Journal, criticizing the defects and weaknesses, and inconsistencies, of the peer review process?

Sir DAVID PHILLIPS. Again, the peer review process is much discussed, much criticized, and also vigorously defended as the best system that we have got. I think, like so many other things, there are systems which operate really very well in times of expenditure, of increasing expenditure, but which begin to show up less well in times of contraction.

One of the problems with the peer review system, perhaps, is that it tends to be a conservative system and if funds are declining, then existing research is continued and it is less easy to be innovative. Consequently, people think, to some extent, of getting away from the peer review system and moving to directed programs. There, again, identifying what program one wants to direct funds into depends upon some assessment and that is often, if not always, done by the assessment of peers.

So I am sure these kinds of discussions go on in the United States. They certainly go on in the United Kingdom.

The trend in the United Kingdom, which I think is very opposite to the considerations of your committee, is to bring the private sector—the private sector of industry—much more involved in these processes than it has been before. And if I could summarize in a sentence, some of the principal objectives of my committee as I see them, they are to break down whatever barriers may have existed between the publicly funded science based and private industry as much as possible so that there is a close interaction between the two and that judgments can be formed which reflect the views on both sides.

Mr. FUQUA. What do you see as the long range implications of the reductions in funding for basic research in the U.K., say, into the next century? Do you think that your country is going to lose some of its strengths as a result of not participating in basic research or do you see it as not having any impact? I know that there is a group I think called "Save British Science" that has a contrary view that expresses themselves that they, as I understand it, feel as though that this will have detrimental impact on the British Government and its position as a world leader.

How do you view that?

Sir David Phillips. I view that with a great deal of concern. I think your starting premise that funds are being positively cut is slightly unfair to the Government position, which has been, as I said, to try to maintain, to retain, the present level of science funding. And given the pressure on other Government programs, which actually have been positively cut, I think the Government record in trying to maintain the science budget is creditable. But at best, it's maintained one-half of the dual support system at roughly level funding while putting the other half under pressure. Admittedly, it has tried to instruct the universities to make its economies in those parts of their activities which were not directly in support of science, and that they should also try to maintain.

Mr. Fuqua. Get more research per pound.

Sir David Phillips. That is right.

Now, one of the difficulties, as I see it, is that maintaining science at a constant level with level funding sounds all right in some ways, but probably neglects some of the intrinsic qualities of science. One of our problems is that the scientific frontier continually expands. The potential for doing new and potentially important work continues to increase. The intrinsic dynamic of science, you could say, is for exponential growth. And that, indeed, is what most scientists working today—at least most medium to senior scientists working today became accustomed to in the 1950's and 1960's.

The question is, can we hope to maintain science in a healthy, vigorous state with more or less level funding rather than growing funds? That, seems to me to be——

Mr. Fuqua. But with inflation you would have really a net——

Sir David Phillips. That is even allowing for inflation.

Mr. Fuqua. That is allowing for inflation.

Sir David Phillips. What I have said allows for inflation.

Mr. Fuqua. OK.

Sir David Phillips. That is, it seems to me, an almost impossible task given the relatively long scale, long-time nature, of science—one can't simply switch it off when something looks better and deflected into something new. It looks as though one does need to have science continually growing.

I am not here trying to pretend that one should not also be very interested in managing it as well as possible. But growing at least with the growth of the economy seems to be perhaps the way to go about it. I don't know even whether that is enough.

So one has to worry about the possibility of managing science with level funds and to what extent that is made either easy or indeed possible by growing funds, and one also has to wonder about the expectations of scientists and to what extent they are concerned about and adjust their activities because of apparent Government policies.

One of the major worries in the United Kingdom at the moment is that although there have not been positive cuts, as I tried to say, nevertheless, the general climate is one of contraction. And this is certainly deflecting talented people from taking up careers in science and is also driving experienced, qualified scientists abroad where their abilities can be better utilized.

All of this against a background in which the United Kingdom, like other Western countries, I believe is going to be increasingly dependent upon science-based industry, but it has to retain some sort of lead over other countries, which are developing very quickly, if it is to maintain its standard of living. I think the whole situation is both difficult and dangerous and alarming.

Mr. **Fuqua**. Mr. Packard?

Mr. **Packard**. Thank you, Mr. Chairman.

Welcome, Mr. Phillips. We appreciate you coming to this hearing and offering your expert counsel and advice and testimony.

Where is Britain placing primarily their priorities in terms of research? Do you see a change—or let me first ask: Is it in basic research or is it a fairly good mix between basic and applied, or are you seeing a change in that priority toward one direction or the other.

Sir **David Phillips**. As I mentioned to begin with, the Research Councils system was set up quite consciously some, I suppose now some 70 years ago, to increase the level of research in areas of perceived economic and social importance. Their programs have always continued that, so that the Agricultural Research Council and Medical Research Council devote a fairly considerable proportion of their funds to what we would, I suppose, nowadays call strategic research with probable economic and social benefits.

The Science Research Council, which was taken out of the Department of Scientific and Industrial Research in 1965 in a reorganization of publicly funded research was initially conceived as the agency which supported basic research almost exclusively.

Over the last 20 years, the Science Research Council, which significantly is now called the Science and Engineering Research Council, has moved more and more of its funds toward, if not applied research, then again, strategic research.

Mr. **Packard**. Has that shifting of direction—over the last 70 years you say?

Sir **David Phillips**. Yes.

Mr. **Packard**. Has that had a major effect upon the competitiveness of your research and the results of your research?

Sir **David Phillips**. I am not aware that the shifts over the last 20 years can yet be seen to have had that effect. But the shifts within the Science and Engineering Research Council, which has meant that its support of engineering science has increased from a rather low level now to more than 20 percent of the total, whereas at the same time, the support for high energy particle physics and astronomy has declined from some 60 percent to begin with to less than 40 percent now, that, I am sure, will affect our contributions to some areas of science now quite quickly.

Mr. **Packard**. Our two countries have produced over the years perhaps a greater commitment toward basic research than most, and the result has been that we have been blessed with perhaps more recognized great scientists in the world—Nobel Prize winners, et cetera.

But a country like Japan, who has really not made a great commitment—at least to this point, even though there appears to be some shifting in that area—toward basic research has done remarkably well in marketable products from their applied research. And as a result, have really placed our two countries—certainly ours here, the United States—into difficult competitive conditions.

How do you account for that?

Sir **David Phillips**. I think the fact that the United Kingdom—and I recognize the preeminence of the United States in basic research but wouldn't wish to comment particularly at this time about that—certainly, I think in the United Kingdom. the educational and social climate has been such to give priority to people who are talented in individual research work. And a consequence of this is perhaps that there has been a gulf between scientists and technologists recruited into industry and those who continued in the universities and research institutes to pursue their own research ideas.

Now, I would want to go beyond that and say I think also we have been a little bit bedeviled—and I will admit, Mr. Packard, this is a hobbyhorse of mine—with adherence to what seems now to be a rather naive way of looking at the impact of science on the development of products and processes. We have thought very much in terms of the simple linear model that some bright scientist has an idea which by a linear process of development turns into a new product or process and economic benefit. Looked at in those terms, I think the United Kingdom. has really quite a good record of having tried to exploit the scientific developments.

If you think of—well, you can think of radar or jet engines, or hovercraft, or a number of different things, where the initial attempt to produce a useful product went quite well. But where the process, I believe, broke down was in the subsequent stages of product and process improvement in development and adjustment to a market—either one that existed, or one that had to be developed. And that, I think, relates to the relationship between industry and the science base in the Research Councils and the universities.

There are mechanisms for moving ideas out of one into the other. But the articulation of the two systems isn't good enough to allow for a continual feedback of difficulties from industry into the research thinking which could lead to the resolution of problems, and so on.

Now, this is, I think, a relatively naive view of it, but my view of Japan—which I have visited from time to time—is that the interaction between their scientists and technologists in industry and those in the universities is much closer, much more intimate. This sort of division of responsibility and objective doesn't seem to exist there.

Mr. **Packard**. I am not aware—and my apologies for not being aware of budget deficits or trade deficits in the United Kingdom; I am certainly aware of them here in our own country—but we are certainly finding here that where funds drive research, and where we lack funds, research suffers. Our commitment to research is being reduced, not because of choice of this committee, certainly, but simply because of the constraints of budget and constraint of funds.

Applied, or the application of our research and being able to make it into an economic benefit for the Government, which in turn provides the funds that we can make available for additional research, both in the basic and application research, I think it tends to be a self-feeding process.

Japan is now being able to concentrate more and more on basic research and they are making that kind of a transition. If your information, as you have traveled, confirms mine, then we will see them move and move more deliberately into a greater commitment to basic research where, in other words, we have the economic success of applied research is now being able to be translated into a greater commitment to basic research. We have done the opposite in many cases where we have made a commitment to basic research perhaps at the neglect of applied, and it has had a reverse effect upon our budget processes, and, therefore, reduced our ability to continue and sustain that commitment to basic research.

Do we need to rethink our priorities in terms of basic versus other types of research in an effort to try to sustain our commitment, our funding commitment, to the seed corn?

Sir DAVID PHILLIPS. You put me, naturally, in a difficult position, as a practicing scientist over the years, until I came into this chairmanship, as I now occupy, of course, I adhered to the view that science was clearly a good thing for subsequent technological development, and so on.

You will be more aware than I am of the fact that it is very difficult to prove that relationship and there are many who would argue that investment in basic research, particularly of an enormous strategic kind—that is, research which is curiosity motivated without any particular indication of eventual application—is a form of consumption rather than investment, which prosperous countries can indulge in.

Mr. PACKARD. I guess that was the fundamental part of my question. How long will we be able to continue to indulge in that?

Sir DAVID PHILLIPS. That, I think, remains, to my mind, a very difficult question to resolve totally. It is not perhaps a terribly good example but if I think of the current nuclear disaster in the Soviet Union, then it seems to me that the consequences of such an event are something which a country—or the world, if you like—has to cope with. A lot of the consequences can, I hope, be foreseen and remedies for them now devised because their exists some kind of international science base which provides the information against which they can be coped with.

I suppose what I am saying is, that this apparent indulgence, this investment in science without regard to its eventual use is in fact increasingly important as the nature of society and the nature of industry changes towards more dependence on science and a higher technology.

Mr. PACKARD. We have others here, Mr. Chairman, so I will try to limit myself to the 5-minute rule, although I have probably exceeded it. One last question, if you permit.

Our two countries have been, I think, generally very cooperative in their commitment and approach to science and research. However, I am certain that there must be considerable duplication of effort and of funds in terms of research that is going on separately in both countries.

In your judgment, are there ways that we can better improve the cooperation between the two countries to eliminate much of the duplication, or should there be joint venture more in research projects, particularly big science? Both countries are experiencing shortages of funds, and that is having an impact on our research.

How can we better cooperate and joint venture or avoid duplication?

Sir **David Phillips**. Perhaps I could begin to answer that by remarking that I have a favorite quotation which I learned only relatively recently—that the rivalry of scholars increases wisdom. And I think competition among scientists is a curiously very important part of their motivation, and so on. Nevertheless, there is scope for collaboration.

Mr. **Packard**. Again, though, that is a luxury we may not be able to continue to afford.

Sir **David Phillips**. Again, I think one has to wonder to what extent such things are intrinsic to the way in which the system operates and the people—because we are concerned here of the productivity of extremely talented people.

Mr. **Packard**. Spoken like a true scientist. Scientists are very proud of the work they do, but still we may find ourselves in a position where it is to the advantage of the scientific community to find ways to avoid duplication and to cooperate.

Sir **David Phillips**. The thing I would want to acknowledge if it doesn't take up the committee's time too much, is that the collaboration, the flow of people to and fro across the Atlantic between the United States and the United Kingdom to my mind has been enormously to the benefit of both countries——

Mr. **Packard**. Unquestionably.

Sir **David Phillips** [continuing]. Over recent years.

I would hope that that would continue.

When we get to the question of collaborating, I think our models there are in the rather big science areas of high energy particle physics, space research, astronomy, and so on, which depend on very large facilities. And there, international experience in setting up such collaborations is quite wide, and I think quite encouraging. There are difficulties, of course. You will know that the United Kingdom recently called for a review of high energy particle physics at CERN because successful enterprises of that kind have a great tendency to grow to do whatever might be considered possible and regardless of anything else.

I think international collaborations can be conservative and difficult to control, and so on. Nevertheless, we have experience in big science which we should build on.

When one gets more to the so-called small science—it is expensive enough—even within a single small country avoiding duplication, managing such science is quite difficult. I think myself we have to rely on maintaining the channels of communication, avoiding blockages to the flows of information, making sure that facilities in one country are open to those in talented research workers in other countries who can benefit from them, and that these flows are kept as equitable as possible. I mean, to be blunt, I would not wish to see all the talented molecular biologists in the United Kingdom working in the United States.

Mr. PACKARD. Thank you very much, Mr. Chairman.

Mr. FUQUA. Thank you.

Mr. LUNDINE. Just one question. This particular series of hearings is exploring the idea of research as an investment. I gather from your answer to Mr. Packard's questions that Britain has attempted to—and in fact that your Board attempts to answer this question—how do you develop or show the accountability?

Sir DAVID PHILLIPS. Producing a strict relationship between the investment in research and products and processes that develop from it is something which committees like mine—and I suspect yours—have broken their teeth on over the years and haven't really quite managed to establish. It's a rather difficult problem.

I think the situation is a little easier if one turns to the other half of the joint product that I talked about earlier on as that which is produced by the University and Research Council system in the United Kingdom. That is, we produce research results and we also produce experienced research workers.

I think it would be generally agreed in the United Kingdom that the most tangible, most easily justified part of that joint product, is the flow of talented workers into industry that the economy needs.

The argument about the actual research is much more difficult.

Mr. LUNDINE. Thank you.

Mr. FUQUA. Mr. Slaughter?

Mr. SLAUGHTER. No questions. Thank you, Mr. Chairman.

Mr. FUQUA. Thank you very much, Sir David. We thank you for sharing with us your thoughts this morning and we look forward to seeing you again, and we look forward to visiting with you later this afternoon.

Sir DAVID PHILLIPS. Thank you very much, Mr. Chairman.

PANEL: DR. RUSTUM ROY, DIRECTOR, SCIENCE, TECHNOLOGY AND SOCIETY PROGRAM, AND EVAN PUGH, PROFESSOR OF THE SOLID STATE, AND PROFESSOR OF GEOCHEMISTRY, PENNSYLVANIA STATE UNIVERSITY, UNIVERSITY PARK, PA; AND DEBORAH SHAPLEY, JOURNALIST, AUTHOR, FORMERLY WITH NEWS & COMMENT STAFF, SCIENCE MAGAZINE, AND WASHINGTON, EDITOR, NATURE MAGAZINE, WASHINGTON, DC

Mr. FUQUA. Our next witnesses will appear together—Dr. Rustum Roy and Ms. Deborah Shapley.

To accommodate Ms. Shapley, we are pleased to hear from you at the beginning.

Ms. SHAPLEY. I had planned to follow Professor Roy's testimony if you want to do it that way.

Mr. FUQUA. OK, why don't we go ahead and do that if it will accommodate you. I understand you have a time constraint.

Ms. SHAPLEY. Yes, but I think it will be all right.

Mr. FUQUA. OK, I hope so.

Ms. SHAPLEY. Thank you very much for the opportunity.

DR. RUSTUM ROY

[A biographical sketch of Dr. Roy follows:]

Rustum Roy is Evan Pugh Professor of the Solid State and Professor of Geochemistry at The Pennsylvania State University. He also serves as Director of the Science, Technology and Society Programs of the University.

Professor Roy is a crystal chemist specializing in the design, synthesis and preparation of new materials. He is the author of some 400 papers and several books. His work has been recognized by various awards and by his election to the National Academy of Engineering, and as a foreign member of the Royal Swedish Academy of Engineering Sciences and the Indian National Science Academy. Professor Roy has been deeply involved in professional and educational innovation. He has been a major figure in the creation of the national interdisciplinary materials research community, by organizing national meetings and analyses of the field, and by being the principal architect of the Materials Research Society and the Materials Education Council. At the University, besides being the founding Director of the University's interdisciplinary Materials Research Laboratory for a quarter century, he played a major role in establishing geochemistry, and later solid state science as academic disciplines on the campus. Most recently he has played the same role in creating and directing the Science, Technology and Society Program which covers the widest spectrum of disciplines. His innovations in the creation and delivery of new educational teaching materials have international significance for both the materials science and STS fields. Besides his research, his current efforts are focused increasingly on the technological literacy of the general population via the STS route.

Professor Roy has been involved in science policy matters for two decades. He served three terms as a member of the Pennsylvania Governor's Science Advisory Committee. At the Federal level he has served on many panels, Boards and Committees of the NAS-NRC-NAE and the National Science Foundation. He has been a consultant to some industries continuously for over twenty years. He has studied and written widely on matters of ethics and values. He was first chair of the National Council of Churches' Committee on Science, Technology and the Church, and in 1979 he was selected to give the prestigious Hibbert Lectures (in its Centenary year) in London, England, on the relationship of science and technology to contemporary religious insight.

STATEMENT OF DR. RUSTUM ROY

Mr. ROY. Mr. Chairman, I am grateful again to appear here because I do have that perspective of that Wall Street Journal ad on April 8, and Mr. Grace's advertisement yesterday in The Post entitled "Debt Deficit and the Death of a Republic." When the Wall Street Journal puts those three things together, I would be taking it very seriously. You are looking at the investment issue, and I think the luxury of us debating the fine points of where to put money is really—it's past, the time is past for that. So my comments will be discussing investment in technology and science. I will use that consciously in that order because I think that is the right order and because, as Mr. Packard was hinting, we are sitting under a Damoclean sword of those deficits and trade deficits—and I have just come from a major technology meeting at which attendees agreed that there is no end in sight of our trade imbalance in the technology sector.

Now, Ms. Shapley and I, as you know, have been updating the Bush report also. I start by noting that there is a radical change in the position of the United States—between 1945 when Mr. Bush wrote to President Roosevelt and the present. At that time, our technology was the unchallenged leader. We had no competitors, they were flat on their backs. But our science was very modest. In 1985, it is exactly the reverse. We don't have a single area of technology where the Japanese can't overtake us at will. I use that term advisedly. Our science is now unchallenged. We are really pretty good. We won all the Nobel Prizes in science recently, and so on.

Mr. Lundine was asking can we get a quantification of accountability. I would say that accountability—the buck stops here. Here with the Congress is where the buck stops. It's your human judgment. We are not going to have any mathematical tables to help and we must make a change.

When Mr. Bush wrote to President Roosevelt, he institutionalized American science in a new way. We got all kinds of NSF's and NIH's and all of the other agencies emerging from the report. I believe this committee—you are facing the same kind of responsibility, a new reinstitutionalization. You cannot leave it to incremental changes in the present institutions. Out there on the Jefferson Memorial it says that scarcely would we expect a grown man to wear the clothes which he was outfitted as a boy. And so we need to have changes in institutions and laws. I believe that time has come.

Mr. Fuqua has set up these hearings to make a major change. And I feel that a signal to the Nation will go out from this committee's work. Are we really going to reinstitutionalize—and I mean that absolutely seriously—or are we just going to play incremental games?

So the first thing I say, if I read back through my written testimony, is on the issue of the linguistic hegemony which has been exercised by the word "science", and "basic science". It has been a dangerous trend to equate the word "science" and "basic research" with all the good that comes, and that leads to products and jobs, et cetera.

I have therefore tried to introduce two new words which I heard Mr. Phillips talk about as "strategic science." I call that telestic—purposeful science; and its opposite I call; atelestic science. And if we don't separate those two we get into all kinds of muddles in discussing investment.

The second linguistics point I make is, that science is very different from technology. Mr. Baruch, who used to be Assistant Secretary of Commerce under President Carter, has often written stuff about this saying that the human communities which practice science and those that do engineering, their values, their products are radically different. Now if the Congress keeps funding technology under science, we will be depriving ourselves of the human connections to a completely different engineering and technology community that really produces the goods which you are looking for, as a return on an investment.

Many scholars now ask the question, "What is the relationship of science to technology?" And, in fact, in our book we use a metaphor which is shown in my written testimony. Present national "science" policy is represented as the one-tree theory. That theory claims that if you planted a tree of science you will get the fruits of technology. That is complete hogwash. The fact is that there are two separate trees: one of technology and one of science. Technology is 10,000 years old; science, 300. Science produces flowers, Nobel

Prizes, beautiful information, et cetera. Technology utilizes the products of science, but technology also needs venture capital: it needs labor, it needs infrastructure, and it needs science. But technology is a science-eating tree. It is not a science-producing tree. And what we need most, today in the United States are science-utilizing trees.

Now, a few people may contest this, and I hope, Mr. Chairman, if you do have people contest it that you ask them to produce some evidence. The historians have given us their view, as reasonably unbiased referees, that science may reasonably be called applied technology; not the other way around as conventional wisdom has it. Science is applied technology. That is Professor Price's view—the arrow of causality, he says, leads from technology to science; not the other way around. Very seldom does science lead to technology.

So if we look now at your question, what shall we do about science policy as investment? I give you two metaphors. I have given you the tree metaphor. If you want to feed the tree of technology, you only need to provide access to science. But that science now is grown all over the world. The British are the best source of science, the most cost-effective source. We should rent a Brit. They are the persons who produce science cheaply, cost effectively. Why do we waste money on our science here? In a sense, we can rent Brits. They are producing the stuff our technology needs.

What American needs now is to get serious about making science useful, putting it to work to make industry and technology.

I have a second metaphor which is the metaphor of the football team. This is NFL draft time. Now, suppose Mr. Gibbs went to the owner, Mr. Cook, of the Redskins, and said what we need are six more quarterbacks. We have one damn good quarterback, and two or three other passing quarterbacks, and what we need are three more quarterbacks. If you have good quarterbacks, get more quarterbacks—that is the kind of advice many scientists give.

Yet Dr. Yannevar Bush, in his report, said that, science is a member of a team. He said that science cannot be of value except as a member of a team. The United States has mistaken a bunch of star quarterbacks for a football team. A football team needs receivers, it needs blockers, it needs place kickers. Because without that, guess what's happening? All of the passes of our star passers are being intercepted and the competitors are running them back in the other direction. So what is happening is that we have a football team of star scientists throwing passes like mad, to the Japanese and German technologists who use them to make new products. I am advocating a balanced team of science and technology.

This is a perfect way of looking at the present U.S. science policy.

Now, how does this translate into policy for today? If you look at the stool I have drawn in my written testimony, we have a three-legged base on which our good technology and science policy should stand. One leg is science, it's in excellent shape. Two is technology—in lousy shape. But three, in the worst condition is science education.

Sir David referred to the question about what we should do about education. Are we educating the right people and giving them the right attitudes? I submit that in the United States we turn out excellent engineers. But most of our scientists are too decoupled from the rest of the system. But the worst is that our public is decoupled from the technological world. And what I urge is, that we first really make a technologically literate community.

Congressman Ritter on this committee keeps urging us to look at the risk issue. I believe you might get better return on investment by investing in the humanities. They could change the country's attitude to risk, such as the fallout of the Soviet nuclear accident. If we could change our attitudes to a kind of deep sense of understanding our technology, that will be worth far more than a lot of additional Supercolliders to U.S. technology.

You have asked in your outline: "As a rationale shifts, do we need to shift in policy?" The rationale for science is already shifting from only supporting pure knowledge to supporting useful. Indeed I submit, the rationale has already shifted dramatically. I have given you evidence in some appendices to my testimony where in the last few issues of Physics Today, within the science community a civil war is starting. The particle physics types figure that they ought to spend $6 billion on a Supercollider. And the argument by the rest of us who are just as good basic scientists, but connected to the team of U.S. technology, is no. The interest on $6 billion is $600 million. The cost of running that damn fool thing will be $1.5 billion a year. And the chutzpah of a person coming in here and asking you to give the SSC $6 billion while giving $10 million to National Science Foundation for Engineering Research Centers. I hope you will agree that it takes a certain amount of chutzpah to say: "Give us $6 billion and give to these serfs, the much larger member of engineers and other scientists, $10 million to help U.S. industry."

I mean, it boggles the mind—when the $10 million is connected to reducing good materials, biotechnology, better chemicals. The only good part of our technology in the balance of trade is our chemical industry. It is saving us to the tune of $10 billion annually right now. Most of our other technologies show a negative trade balance.

So, why is the United States doing this crazy thing of saying, we need even more good atelestic basic science, totally unconnected basic science like the Supercollider?

So I feel that one has to make a few major changes. First, we have to re-rationalize the total technical education question. We must get better technological literacy for all, and people being trained as technicians and so on. Second, on the technology issue, I can't debate the details here. One of your colleagues, Congressman Brown, introduced H.R. 481 a few Congresses ago, in which he suggested that we create something like a national technology foundation.

My recommendation to you is that if Bush in 1945 instituted a National Science Foundation, I would hope that this committee would institute as major a change—a separate funding institution with separate but equal status, for technology which is connected to the economic base of our country. If the Congress neglects to do that, I feel that the signal will go out it is "business as usual." If you don't make a change in the institutionalization of the applied, useful, just as basic science is, I think the Nation will have gotten the wrong signal.

Thank you, Mr. Chairman.

Mr. Fuqua. Thank you, sir.

[The prepared statement of Dr. Roy follows:]

Testimony of Dr. Rustum Roy

Director of the STS Program
and
Evan Pugh Professor of the Solid State

at

The Pennsylvania State University

before the

U.S. House of Representatives
Committee on Science and Technology

May 1, 1986

QUALIFICATIONS OF WITNESS

Dr. Rustum Roy, one of the country's leading materials scientists, is Evan Pugh Professor of the Solid State, Professor of Geochemistry, and Director of the Science, Technology and Society Program--all at The Pennsylvania State University. He was founding Director of the University's Materials Research Laboratory (the nation's largest) for 25 years.

He is simultaneously a currently active scientist with some 400 papers, dozen patents and several books to his credit, a senior research administrator intimately involved with U.S. industry, and a science policy analyst. In the latter capacity he has for 25 years served on major National Research Council policy committees, served four Pennsylvania Governors as a senior member of the Governor's Science Advisory Committee and was a Science Policy Fellow at the Brookings Institution in 1983. He has written widely in the science policy field especially on the subjects of interdisciplinarity, university-industry interaction and alternatives to proposal peer review and the absence of a U S. technology policy.

Introduction

Mr. Chairman, your extensive series of hearings on a long-range look at the Science and Technology Policy for the United States is nearing the end of its public phase. This is a most remarkable and commendable effort. But I am sure that the experienced Congresspersons on the Committee are much more aware than I, that the biggest danger of all that you face is losing sight of the forest for the trees.

In the very structure of your agenda for the study one can see the footprints of the reductionist scientific worldview in which we are immersed. Will the analysis of the **parts**: The Goals, The Institutional Framework, Education, Funding Levels, Funding Mechanisms (the section titles from the Table of Contents of your Agenda document), give you an answer to the **whole**--a new Technology Policy for the nation. I believe not. It is very important to remember that while

SCIENCE SEEKS THE TRUTH but by its own canons

SCIENCE CAN NEVER GIVE THE WHOLE TRUTH

That is the job of human beings exercising judgement. In matters of national Technology and Science Policy it is only your judgement which can yield the answers--correct or otherwise.

Your staff has asked me to focus today on the "Pay-off from Scientific Research." I believe this to be the crux of your entire exercise and am honored and pleased to be able to offer my insights on this matter. My experiences in research and in science policy are outlined in the witness qualification statement.

Your Agenda document, p. 11, D, 1, 2 and 3 asks three very important questions:

1. What should the rationale for the Federal support of T&S be?

2. To what extent is the support of science comparable to support of the Arts and Humanities?

3. As the rationale shifts what are the effects on technology and science funding levels and mechanisms?

I will address mainly this theme "The Pay-off from Scientific Research" (i.e. Section D), and will comment only briefly on Accountability (Section E).

"Institutions Must Advance to Keep Pace with the Times"

Two miles from here there is inscribed on the walls of the Jefferson Monument more relevant advice than many of us will be able to give you.

> *I am not an advocate for frequent changes in laws and constitutions, but laws and institutions must go hand in hand with the progress of the human mind. As that becomes more developed, more enlightened, as new discoveries are made, new truths discovered and manners and opinions change, with the change of circumstances, institutions must advance also to keep pace with the times. We might as well require a man to wear still the coat which fitted him when a boy as civilized society to remain ever under the regimen of their barbarous ancestors.*

Mr. Chairman, you have set yourself the task which might be called the updating of Vannevar Bush's report to President Roosevelt. As one who, with my colleague Deborah Shapley (1), used that very same approach three years ago, I can say with some conviction: The situation of the U.S. is so radically different today compared to 1945 (or 1965 or 1978) that looking back nostalgically could be extremely dangerous. In 1945 the U.S. was the only undamaged technological power in the world with absolutely no competition. Today there are but a few niches where our technology is not surpassed or seriously challenged. In 1945, we were not a major player in fundamental science: today we are unchallenged leaders. In 1945 Americans had the means and the drive for demanding education in science and engineering; today other occupations are getting the best in both quality and quantity. Since 1945 the concept of "Science" as Endless Frontier unalloyed blessing, indeed omnipotent national saviour, has been dealt some chastening blows: the environmental movement, unemployment, population issues have been linked to "science." Except among Marxist idealogues, a more balanced appraisal of the potential good and harm is now the vogue. To meet this radically transformed setting, our present institutions--for giving policy guidance, for funding, and for doing science are clearly just plain in need of replacement. Tinkering with minor adjustments by your Committee will condemn the nation to a certain slow decline. We need major and radical re-institutionalizing of science to meet our new situation just as Bush did in 1945 for his new situation. This Committee cannot have done its job unless it comes up with major new structural changes for the management of national R&D.

Science and Technology CANNOT be Treated as One

I begin by raising a huge red flag about the linguistic confusion which will prevent you from coming to any fruitful decision if left unresolved. To equate, elide or confuse science with technology as though they were the same thing will leave "confusion worse confounded." No linguist, historian, sociologist, allows us to use these terms interchangeably as indeed they are in contemporary society. They are very different human activities conducted by different communities. Of course, this Committee is too knowledgeable and sophisticated to be confused, but eternal vigilance must be exercised to prevent and roll-back the creeping hegemony of "science's" encroachment over engineering and technology which has occurred over the last four decades. The atom bomb, radar, etc., were triumphs of U.S. **technology**, utilizing German and British sciences, yet they are often cited as examples of the opposite.

It is absolutely essential that this Committee recognize that there are three separate human communities--science, engineering technology and "science"educators--which perform three separate functions in the area of policy over which you have oversight. In Table 1 I have pulled together some quotations which make this point regarding these distinctions and the relation of science to technology, explicit. And Figure 1 is an attempt to encapsulate the consensus view, **by those that study or write in the field of these** relations (Refs. 2-5).

My First Recommendation to the Committee

*IT IS ESSENTIAL THAT THE COMMITTEE AFTER WHATEVER DELIBERATION IT NEEDS, KEEP SHARPLY IN FOCUS THE **DIFFERENCES** BETWEEN **SCIENCE** ON THE ONE HAND AND **TECHNOLOGY** ON THE OTHER. SINCE IT IS CHARGED WITH MAKING POLICY FOR BOTH. FAR FROM BEING IDENTICAL, OR EVEN ALWAYS POINTED IN THE SAME DIRECTION, POLICIES GOOD FOR ONE MAY BE HARMFUL FOR THE OTHER AND LIKEWISE TO EDUCATION. THIS WILL BE ESPECIALLY TRUE AS THE BUDGET CRUNCH HITS.*

Setting Priorities Among Different Pay-offs in the Eighties

We turn now to the central focus of today's session. Is the Federal "investment" in science a "profitable" or "valuable" one? The expression "pay-off from an investment in scientific research" recurs throughout the Committee documents. I have little to add to the attempts at being quantitative. The

TABLE I

SCIENCE IS APPLIED TECHNOLOGY
(not vice-versa)

It has often been supposed, on no real basis but assertion, that science can in some mysterious way be applied to make technology. Quite commonly it is said that there is a great chain of being that runs from basic science to applied science and thence to development in a natural and orderly progression that takes one from the core of science to technology.

Price disputed this view vigorously.

The arrow of causality historically is largely from the technology to the science.

D. deSolla Price (1,2)

Indeed, tracing the evolution of science in the early decades of this century, Derek deSolla Price found that all progress during this golden age of experimental physics seemed to depend on very ingenious craftsmen "with brains in their fingertips." As Price put it, "From time to time, perhaps even predominantly, experimental craft and art provide an exogenous force that moves science rather than does its bidding." He concluded that "the arrow of causality historically is largely from the technology to the science," although he shrank from saying that science is simply "applied technology."[13]

M. Kranzberg (3)

The cultures of science and technology are thus almost opposites—a fact we either ignore or deny when we establish agencies under the rubric of "science and technology." If we are to make and implement sound public policy regarding science and technology, we must understand these differences and capitalize on them rather than deny them. Indeed the linking of science and technology that we do routinely may be the first hurdle of denial that we must overcome.

J. Baruch (5)

SCIENCE & TECHNOLOGY ARE DIFFERENT HUMAN ACTIVITIES

If we accept the premise that every human activity has some aim, end, or goal, let us say that the goal of science—as defined by the scientists themselves—is understanding nature, and that the goal of technology—as revealed by the actions and statements of the technologists—is making useful things. Let us say further that the scientific goal is understanding for its own sake, meaning for no other purpose than understanding; but the goal of technology is making useful things for the benefit of man and the needs of society, meaning a myriad of purposes defined and directed by men and society. Science, therefore, is, or should be, free, which means not controlled by men or society other than the scientists themselves. Technology, on the other hand, is subject to limitations, many of them economic or social, which [illegible] imposed by other than the technologists themselves.

These propositions, it seems to me, are supported by the preponderant weight of historical evidence. If there are technologists who make useful things, not for themselves or others, but solely for the things themselves, those engineers are completely anomalous, not to say anonymous. The technologies are utilitarian, pragmatic, practical, and responsive to felt needs. What they make is always for the sake of something else, another purpose, or a line of other purposes. Science, on the other hand, has within the past three centuries established a general freedom from purposes imposed by, say, church and state.

M. Kranzberg (3)

Science has as its constituency the general public Within the science culture, the results of the science process are considered free goods. While secrecy may be observed to ensure priority, once results are in, publication is the rule. Institutions that fund the major work in science, universities, and fellow scientists measure scientists, among other things, by how well and quickly they disseminate results. Science is among the most open of activities. The output is knowledge and understanding, and this output is most often embodied in publications and in the process of teaching.

Engineering and technology are very different from science. There are, of course, engineering scientists engaged in engineering science—just as there are biological scientists and nuclear scientists—and they follow the science culture and leave as their legacy public scientific knowledge. Almost all engineering, as is the case with technology, however, reaches its expression in things and services. Only incidentally does engineering or technology result in a legacy of knowledge. That such knowledge is a by-product does not demean its importance; it merely identifies it as irrelevant to the driving forces in the culture and the constituency.

J. Baruch (5)

4 (b)

OUR WEAKEST LINK IS IN DIFFERENT OPTIMUM "SCIENCE-TECH" EDUCATION FOR (a) THE PUBLIC, (b) SCIENTISTS, (c) ENGINEERS

Frontier with the Educational Establishment

Perhaps the greatest challenge, and at the same time the greatest opportunity that we, as a nation, face is in the area of education.

The latest National Research Council study entitled "The State of School Science" shows that:

1) Only one-third of the nation's high schools offer more than 1 year of mathematics or of science.

2) At least half of all U.S. high school graduates have taken no more than 1 year of biology, no other science, and no mathematics beyond algebra.

3) Only 105,000 U.S. high school students study any calculus at all, while 5 million in the Soviet Union take 2 years of it. The Chinese situation is similar to the Soviet one.

4) Japan graduates five times as many engineers as does the United States.

5) Shortages of supplies and equipment in the schools have, in the last decade, cut by more than half the exposure to any form of laboratory experience of even those students who take science.

And there is more:

6) At present, only 75 percent of those enrolled graduate from U.S. high schools; in some areas of the country this percentage drops to 55 percent. In contrast, 98 percent of all Soviet youth complete, successfully, a 10-year secondary school program generally agreed to be substantially more demanding than ours.

7) Even worse, recent studies show that 20 percent of those who do graduate from U.S. high schools are illiterate and unable to function effectively in our society.

8) And, across the board, average scores of high school seniors on Scholastic Aptitude Tests have continued to fall. The average score for which these tests were standardized is 500. In 1979 the national averages of those who took the tests were 427 and 467 for verbal and mathematical aptitude, respectively; College Board estimates suggest that if all 3 million U.S. high school seniors had been tested the results would have been 368 and 402, respectively. Most serious of all, and contrary to popular belief, the fraction of those scoring in excess of 600 on these tests is now also decreasing

(A. Bromley (6)

The culture of scientific manpower is fundamental to everything we do. The term is being used here in a very comprehensive sense to include the entire professional life-span of scientists as well as the recruitment and early development of scientists, but space is limited and I shall simply propose that

1) current science education distorts the recruitment and development of scientists,

2) the current reward system within the profession distorts the distribution of scientific talent within the profession,

3) and both distortions are to the detriment of the scientific community and the public's well-being.

A. Harrison (4)

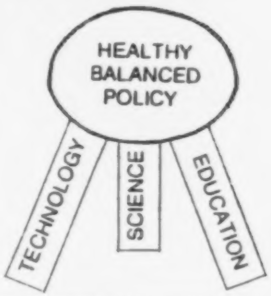
This question of the relations of science and technology is not simply of historical interest nor a matter of professional identity to scientists and engineers. Actually, the interactions between the two are of importance in determining our ability to meet the needs of mankind with the help of our sophisticated science and technology. Study of the dynamics of their relationship might help us make them more creative, and also might make the education of embryonic engineers and scientists more meaningful in meeting the problems of the future.

M. Kranzberg (3)

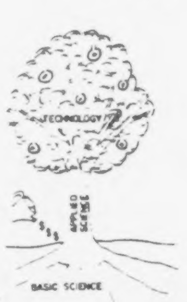
Figure 1

SCIENCE, TECHNOLOGY POLICY
DEALS WITH THREE SEPARATE HUMAN COMMUNITIES

- **ENGINEERS/TECHNOLOGISTS**
- **SCIENTISTS**
- **"SCIENCE"-EDUCATORS**

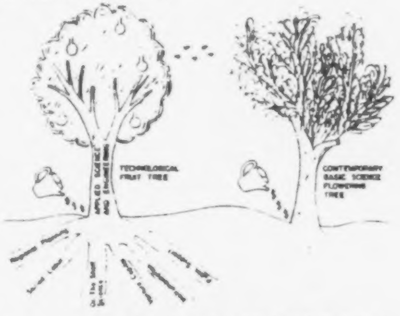

HEALTHY
BALANCED
POLICY
TECHNOLOGY
SCIENCE
EDUCATION

WHAT IS THE RELATION OF SCIENCE TECHNOLOGY?


TECHNOLOGY
APPLIED SCIENCE
BASIC SCIENCE

The conventional wisdom about the relationship between science and technology is represented as a single tree. If its roots, which are basic science, are watered, then the fruits, which are technology, will grow automatically. This widespread belief is disproved by the postwar experience of Great Britain (which has watered its basic science roots but has difficulty growing technology) and Japan (which has little basic science but grows technological fruits).

(see Ref. 1)


APPLIED SCIENCE AND ENGINEERING
TECHNOLOGICAL FRUIT TREE
CONTEMPORARY BASIC SCIENCE FLOWERING TREE

A more accurate metaphor is two trees, one for basic science and one for technology. This recognizes their distinct intrinsic character and the fact that they are nurtured separately by separate policies. Each tree, properly nourished, produces its own fruit: the basic science tree grows Nobel Prizes, and the more complex applied science-engineering-and-technology tree grows technology. Since the trees depend on each other as well, through bees that cross-fertilize, this model emphasizes the importance of establishing policies that nurture not only the technology tree in its own right, but the linkages between the two.

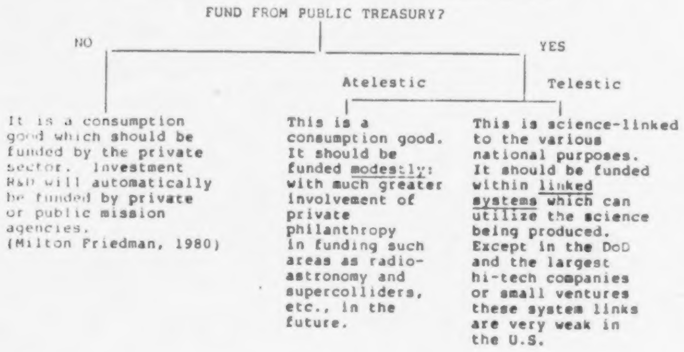
4 (c)

Committee has wisely convinced the OTA to assess the literature on attempts to **quantify** the ROI and even extrapolate such into the future. Their draft document comes to the same conclusion that most experts have come to, that is, that quantification of ROI is an elusive and futile task, and that any numbers we use tend to mislead.

Investing in Telestic (Purpose-linked) and Atelestic (Unconnected) Science

Over the last few years I have introduced the terms telestic (with a purpose, towards an end, etc.) and its opposite, atelestic, to make an essential distinction (for policy purposes) between two types of basic science. Atelestic science is **exactly** analogous to the arts and humanities. Examples of atelestic science include particle physics, radioastronomy, a great deal of "basic" mathematics, chemistry, physics, biology. The argument that "one never knows" what'll pay off, is intellectually dishonest and no longer sustainable. It applies with equal force to art and the humanities. As an "investment" it would be analogous to investing in penny stocks or the lottery because "one never knows." Atelestic science is in the language of economists, a consumption good. It is **NOT** an investment at all. The "pay-off" **may** indeed be there, in terms of a more (or less) vibrant-culture, greater (or lesser) spiritual health, etc. I believe that any modern society must fund such science from the public exchequer at **some** level. Milton Friedman (1981), as you know, disagrees, and I have not seen any reasoned debate by scientists arguing the case as to why or how much the public should invest in such atelestic research. I reproduce below the top of the algorithm for research project selection I presented here on April 8.

A General Algorithm for Research Project Selection



It is a consumption good which should be funded by the private sector. Investment R&D will automatically be funded by private or public mission agencies. (Milton Friedman, 1980)

This is a consumption good. It should be funded modestly: with much greater involvement of private philanthropy in funding such areas as radio-astronomy and supercolliders, etc., in the future.

This is science-linked to the various national purposes. It should be funded within linked systems which can utilize the science being produced. Except in the DoD and the largest hi-tech companies or small ventures these system links are very weak in the U.S.

My position is that atelestic science should receive modest funding but that in an era of privatization, that we have here an obvious candidate for a much greater participation by private philanthropy. Neither the idea nor the practice is new. Great research institutions--The Carnegie Institution here in Washington with its many laboratories comes to mind--have been and are financed privately. Many major facilities for astronomy have been built with private funds. Just as great art is funded to a great extent by private philanthropy, I have yet to find a single argument against similarly funding great atelestic science. It would return science to its much freer and more creative days. Moreover many industrialists today have amassed fortunes **from science and** technology which they should be proud to redeploy in heroic monuments to science: New superconducting supercolliders, new synchrotrons, new pulsed neutron sources running from $100 million to a few billion are excellent candidates for total or partial private funding. Certainly an excellent case could be made that Congress would only match the funding of atelestic science by providing **operating funds** for approved facilities for which private capital funds were gathered.

The competition between Big Science and Little Science is heating up, all over the world; we may be on the verge of civil war! Most of Big Science is atelestic. And the **evidence** is that even within the Physics community the community realizes that Big Science **cannot** deliver returns to society, and that funding it at the levels it has been used to will ineluctably damage all other sciences. I attach copies of the controversy even within the pages of the house organ of the Physical Society as recently as last week. I also attach comments by Dr. A. Penzias, Nobel Laureate in Physics, Vice President of Bell Labs, where he stresses the point that we must not confuse telestic science (quantum mechanics) with atelestic science (such as particle physics).

From the point of view of pay-off on an investment in science one would have to be Alice in Wonderland to believe that **any additional** allocation of funds to atelestic science such as those noted above could be justified. Encouraging privatization is an excellent middle ground.

The more relevant and more difficult problem remains. Is the present ratio of allocation to atelestic science already too high, and what should we do about it? It is my position that the United States allocation of funds to atelestic basic science is substantially too high, and that this high ratio has a negative impact on our telestic science, and our engineering and technology. In answer to the questions in your Agenda: The rationale for public funding of science is shifting away from making the U.S. a major player in the world Nobel prize competition (i.e. for national prestige) towards supporting declining industry in most technical fields. Yet this shift--within the "attentives" community lags reality by several years because most have yet to discover the dire straits of our industry.

In the past (indeed in this Administration even very recently) it was argued that winning Nobel prizes, being #1 in science led to being #1 in engineering and technology. Of course that is now know to be totally false yet it is presented as the rationale for "science" policy. Mr. Keyworth's view is reported in Physics Today (Apr. '85, p. 63) as the following:

> He was convinced, as are the members of the President's Commission on Industrial Competitiveness, said Keyworth, that science leads inevitably to innovative technology, and thus to prosperity.

How can any sane policy emerge if based on such an erroneous premise.

I will not labor the proof of the absurdity of this thesis (see Shapley and Roy) but simply ask how if this were true, that the Japanese who have won only one thirtieth the number of Nobel prizes as have we, should be so outstanding in technology, and the British who have won twice as many as we (per capita) should have suffered such an industrial decline.

WHEN IT COMES TO RETURN ON INVESTMENT IT IS YOUR SCIENCE-UTILIZING CAPABILITY THAT COUNTS. THE SCIENCE-PRODUCTION IS TOTALLY IRRELEVANT TO ECONOMIC RETURN. INDEED IT IS ALMOST CERTAINLY COUNTERPRODUCTIVE. FOR A NATION TO HAVE SO MUCH SCIENCE PRODUCTION AND SO LITTLE SCIENCE-**UTILIZATION** CAPABILITY.

I believe a more useful way to approach the subject of investment strategy in R&D is to put oneself in the position of an owner of a football team like the Dolphins or Steelers. The position of Mr. Keyworth and this Administration, and indeed of many others in the science community, can be described in the following parable--which is to appear shortly in a national newspaper.

U.S. "SCIENCE" POLICY: A TEAM OF QUARTERBACKS ONLY

Now I ask you, would you take any football coach seriously who, having recruited three star passing quarterbacks, and having no tackles and very poor pass receivers, went to the owner in desperation asking if he could trade someone for yet another . . . passing quarterback.

Without any exaggeration whatsoever that is exactly what is going on these days in Washington's Technology and Science Policy making--and no newspaper is able to penetrate the smokescreen of jargon and clever linguistic obfuscation used by some of my scientific colleagues to peddle this pernicious nonsense: that by adding quarterbacks alone you win football games. My football colleague Joe Paterno knows better . . . to win games you need a **balanced team**, pulling together. One reason why our deficit even in the highest tech field of electronics was $12 billion last year is that the U.S. has many separate stars, but no team, in its technology "policy."

Let me translate this rather good analogy, where "football" has been the metaphor for "technology and science policy" and show the reader how we got into this mess. The first trick is to change the **name** of the game, from "football" to "quarterbacking." Thus Technology and Science policy becomes "science" policy. That way you don't really need to discuss technology very much at all. Of course to the public which pays the bill "science" means mainly technology: nuclear power, acid rain, steel factories, and computers. But the boys-in-the-back-laboratory, know deep in their hearts that "real" science means longer and longer equations, and sophisticated instruments looking deeper and deeper inside atoms, gazing at ever-more distant stars, etc. The public believes that the goal of a football team is to win games by carrying the ball over the opponents goal line. Not so the old boys . . . the goal of the game is to throw the ball extremely well and gain fame for the quarterback . . . and well . . . doesn't that always win games? Well, no! The Japanese have proved that to us. But real scientists supported by taxpayer indulgence don't bother with the foreign trade deficit. The public has been sold a bill of goods; that "science" (i.e. well-executed passing) will deliver the goods: jobs, prosperity, long life, health, cleaner air, on and on (i.e. win games). Even if many scientists sincerely adhere to this religious dogma, it does not make it right, nor excuse their lack of concern with the national issues. The fact is that further research on the structure of the moons of Uranus, and the charms of a quark absolutely cannot help with human prosperity or health. What is needed for the latter is improving our "technology," the most disposable word in national policy today. Now technology is the analogue of playing football in our story, while science is throwing passes. Technology's goal is to win games, not just throw passes. It knows that winning games takes a balanced team. Bankers (sturdy fullbacks), venture capitalists and entrepreneurs (place-kickers), well-trained labor and fast running backs to receive the passes (engineers). To complete your understanding of the incredible ineptitude of the present Federal S/T policy you must understand the relation of "science" to "technology." Virtually every analyst of U.S. S&T policy agrees that while the U.S. is very good at doing basic science (just look at our string of Nobel prizes) we simply cannot put that science to work to make new products, new jobs, etc. We are a science-producing culture; Japan is a science-utilizing culture. Our football analogy is uncannily apt. Science does not lead by itself to technology; proof, many teams win games by moving the ball on the ground. A good passing attack can help but is far from necessary and obviously not sufficient by itself. U.S. quarterbacks throw the balls extremely well: but there are no competent receivers. We drop the ball of new technological opportunities. Worse, Japan, West Germany, etc., have learned that all they have to do is to intercept our own passes and with the rest of their technology team, run off to score.

Now a reader might say, well if what you say is correct the needed science policy changes seem simply enough, stop hiring and training more quarterbacks, concentrate on receivers, blockers, etc. The reader would reckon without the narrow self-interest of most scientists who have--as much as any second-generation welfare mother--grown up on the Federal dole. A public holding the idea that research scientists are open to fresh ideas, always on the lookout for the new, eager for innovation concerned about the national welfare would be sadly disillusioned by attending recent Congressional hearings on Science Policy. Talk about 'deja vu'! Here one finds a scene reminiscent of the heyday of the Great Society. We need to expand this . . . more money; new equipment is desperately short . . . more money please; the U.S. must remain #1, in my scientific field (why when it has slipped so badly in autos, steel, consumer electronics, should the nation care about some esoteric branch of science is never explained) . . . send more money. You see, most academic scientists--even those getting hundreds of thousands of dollars of public money every year like this writer--don't think of themselves as playing on Uncle Sam's football team. They think that they are superb passing quarterbacks, stars, virtuosi, prima donnas and the public should be proud to just watch them do their stuff--even if it has no connection to society's needs, i.e. to winning football games.

- - - - - - - - - -

I do not need to apologize for this simple "team" metaphor for Vannevar Bush himself wrote

> Science, by itself, provides no panacea for individual, social, and economic ills. It can be effective in the national welfare *only as a member of a team* [italics added]. . . .

Balancing the SYSTEM

Returning to more formal language, if we see T&S policy as the SYSTEM it is, then clearly we recognize that the chain is only as strong as its weakest link. Or look back at our three-legged stool of T&S. Our science leg is sturdy; our technology leg in most fields is thin and cracked, and our education leg is on the verge of collapse. A distinguished leader of the physics (6) community has provided statistics about the state of science education which should counteract our pride in winning Nobel prizes, but those are quickly forgotten. One recent one is that the median performance in mathematics of high school graduates in Europe is attained by only 4% of the equivalent U.S. population. The science and engineering community, and their representatives on bodies such as the National Science Board, bear a major share of the responsibility for this state of affairs. They did not consider the system as a whole, and advocated or accepted more dollars for research, while innovative science education, was zeroed out in the second Reagan budget. But even today the scientist community is unwilling to look at a **systems** approach. I am saddened to note that the two National Academy of Sciences studies on Chemistry and Physics, respectively, both attended to the very unhealthy situation in science education especially in high-schools. Yet not one study said that in order to balance the **system**, if there were no increases for total research funding, that money should be **taken out of basic scientific research** and put into science education. The Congress must force the science community to set priorities in a zero increase scenario and only then provide increases if available.

Within education I advocate first and foremost programs to change our culture's attitudes to technology. Senator Gore, sharing the same platform with me at Brookings, has referred to the U.S. citizens' addiction to technofix, so that any huckster can sell anything disguised in high-tech hype. We must develop a more evaluative attitude in all citizens. On the other hand, Congressman Ritter of this Committee has been deeply concerned with the public's attitude to risk, and how much these irrational, but no less real, attitudes are costing our economy. There is something you can do about both these attitudes, the cost of which is minuscule. Sweeping this country's education is a grassroots megatrend; it is the introduction of Science, Technology and Society general education to create technologically literate citizens. My title betrays my self-interest in this but I cite it with little concern, because an entire national program to increase STS citizenship education funded via NSF and Dept. of Ed. would not be equal to funding one new engineering technology center.

However, I cannot conceive that the Federal role as pioneer, model researcher, experimenter to supplement the huge state and private investment in science and engineering education, should be less than 15-20% of NSF's budget. While many many scientists would agree with me that this is needed, my position is that as an **investment**, the increase in **education funds** would come prior to or at the expense of expanding research funds.

SCIENCE AND TECHNOLOGY EDUCATION AS FIRST PRIORITY

My second recommendation is: that given the gross unbalance in our total S&T system, the first priority for increased funding should be for science education, second for basic science linked to engineering and technology, and only lastly for atelestic science. Indeed with a fixed budget I would recommend substantial transfer of ***dollars*** *and* ***personnel*** *(our most important resource) from the atelestic science area to the other two areas. By science education I do not mean turning out more engineers and scientists, but raising the level of technological education of all students, and all citizens--lawyers, ministers, housewives, etc. No democratic society as dependent as we on technology can long prosper if 90% of its citizens are totally illiterate of the basics of their own world.*

Reversing the Four Decades of Tilt Towards Atelestic Science

The Federal government's funding for Technology and Science can only be made into an investment if its own investment strategy be clarified. **It is** certain that continuation of the present policies will lead to technological suicide. The first needed change is to substantially tilt the funding back towards telestic S&T, **whether or not any additional funding is made available.** The second needed change is to increase substantially the telestic funding aimed at the commercial sector at the cost of Defense and Space. The arguments based on the spin-off value of such research are totally outdated. Spin-offs, if any, are few and far between and in the approaching technological crisis, personnel in short supply and dollars which will remain fixed or decline must be assigned to telestic science connected to civilian industry and employment.

University-based Research Centers: Only of value if carefully structured

My analogy of the three-legged stool also indicates the other area where the nation can make a genuine investment from which it will get a more certain return on investment than in any other. The re-education of our scientists and engineers in Technological literacy for the **general public** from grade school through college, must result in a **much** greater technological awareness on the part of our scientists, and a much deeper involvement in industry by our engineering faculty. Without this kind of "cultural revolution" within the American university, we cannot recapture the spirit of Yankee "enginuity" which made this country great. As your committee well knows the U.S. has bet a lot more of its R&D dollars on the universities than have our competitors. Has the return on the Federal dollar been maximized with respect to cost-effectiveness? Speaking now as a citizen, policy analyst I believe that compared to the national laboratories, the universities may look okay--especially because of their training functions, yet they are far from optimum vehicles. In science and technology the universities are too uniform, virtually clones of each other. They have demonstrated their failure time and again over the last thirty years to organize themselves to attract industrial support or conduct interdisciplinary work. But all national and societal goals are interdisciplinary. Ergo: most (and there are half a dozen notable exceptions) universities are not a good bet for major involvement in helping rescue U.S. technology and science. Therefore it is with some wry amusement that one reads the sanguine remarks of this Administration's T&S policy leadership. Here is a sample from recent remarks by Mr. Keyworth:

> "The difference between this view of the future and the way we have always pursued science and technology in the Universities is significant, because it will inevitably (sic) force the changes in the way research institutions are now."
>
> "We had $10 million in NSF's 1983 budget for the centers, and we had proposals [submitted] totaling $2.2 billion. That kind of response with new proposals in a matter of months does not occur unless those universities had ideas for it on their own, so that when the Federal program was in place they were ready to jump."

Mr. Keyworth, a neutron physicist, has never been on a university faculty. He could not have talked to any in-harness university researchers or engineering deans or he would have known that all Universities are geared up to produce a proposal at the very mention of a RFP. It is indeed one of the main products of university scholarship!! To use the **submission of proposals** for ERC's as an index of hope for America's revival of technology appears ludicrous beyond words. This misplaced hope in the future strangely ignores the record. In the current issue of a National Academy of Engineering magazine "The Bridge," Mr. Keyworth again cites the six new NSF Engineering Research Centers as beacons of hope. "These six centers" he writes "are like viruses infecting the universities and industries involved." All this as if this the idea of such centers was some brand new idea!! Ten pages further **in the same issue**, Dr. Robert L. Sproull, one-time President of the University of Rochester, one-time Director of DoD's ARPA, gives the history of the materials research laboratories. Funded continuously for over twenty-five years at levels above that of the ERC's, they have certainly not saved our materials industries. Our own laboratory has been in the forefront of advocating and practicing coupling with industry. We are probably the biggest Materials Research Laboratory in the nation, and probably have the highest percentage of industry support. I have obviously been a practitioner of what Mr. Keyworth is belatedly supporting. Yet to you discussing investment in technology and science I would strongly recommend that you hedge your bets and protect your investment. Just funding group research on campus **cannot** do the job. It needs much more firm direction than that and vastly more assurance of stability than most federal programs have had. From my long experience in the field I believe the simplest way to assure the interdisciplinarity and focus on mission perhaps is to establish a new class of **autonomous**, jointly administered centers on campus. We have many models in the USDA and U.S. Geological Survey labs on campus. Just as we in the professoriat must not expect Congress to somehow ignore all the compromises, trade-offs, etc., it must make in passing legislation. So you must know that academic politics is just as constraining, and while proposals promise much by way of societal response, the **performance** record is poor. An extremely simple way of assuring a new attitude to the **relevance** of the research would be to connect the funding of any university-based center to **the amount of hard dollars from industry given for the same center**. This is the very best peer review of telestic science: the number of companies and $ support in some combination.

My third recommendation therefore is that by far the most important investment the U.S. Government can make is a coordinated set of actions to capture for American industry the values of our (and the world's) science-producing capabilities. This set includes (in addition to the education efforts noted above):

1. The establishment of separate agency for coordinating U.S. science-utilizing agencies and laboratories, etc., and funding engineering and technology research in universities, industry and not-for-profits. (The National Technology Foundation of Congressman Brown is a first-rate model, see below.) A new agency is an essential signal that it is no longer business as usual.

2. Funding on semi-permanent basis of fifty to one hundred **"networked** centers." Such centers would be cooperative ventures being a "network of funders" with many companies within a specific industry, the State (or regional government) and (more than one agency of) the Federal Government all contributing. It would be located on a university campus with exceptional, broad strength in the area and able to show a track record of handling interdisciplinary work. The Federal involvement should be proportional to the industrial involvement. The management should be semi-autonomous of the university. The local university should be the hub of a "network of other performers" of research--universities and others drawn from all over the country. Penn State's MRL has evolved this model as the optimum in an NSF-DOD-Industry "National Dielectric Center." Many of its features are appearing in other newer centers.

3. The center should also serve as a pro-active agent for capturing scientific information from all over the world, which can be utilized in the new consortial approach. Funds for systematic international travel and collection of information should be built into the structure.

4. As an investment, this item should take precedence over **any** increases in other fields. In a zero-budget, it will require shifting of funds from atelestic research and concentration in specific areas.

FUNDING TECHNOLOGY-RELEVANT RESEARCH--NEW STRUCTURES IMPERATIVE

Finally I turn to administrative structures to safeguard your investment's potential. To return to my football analogy, trying to get NSF--not its managers, but its constituency--to provide a new balance to the programs and fund more engineering and technology is like getting the "quarterbacking school" to increase the role of place kickers and pass receivers on the team. Atelestic scientists simply cannot be permitted to dominate Federal funding agencies and the media any longer. One way of utilizing the national laboratories more effectively would be to join them to an established University and make them analogous to West Point and Annapolis, National Technological Universities configured to train a new breed of techno-scientist, and do genuine **only**, interdisciplinary research. The irony was lost on Mr. Keyworth that while he was defending the 6 or 7 **billion** dollars for the newest accelerator (SSC), he was discussing NSF's generosity towards Engineering Centers, which cost $10 **million** that year.

You in the Congress have learned to deal with different human constituencies. I urge you to consider that as the basic reason why the funding of atelestic science MUST be separated from the funding of useful and purposive engineering and technology. I am certain that the Congress of all our institutions knows that the mechanisms and institutions of funding are in the long run more important in assuring a good return on investment, than the picking of targets or winners in any one year.

That is why I have been such an ardent supporter of Congressman Brown's bill (once HR481) to establish the National Technology Foundation. There is no need to discuss any details. My own arguments lead in an absolute sense, to the necessity to separate out the funding of telestic science and technology from the funding of atelestic science. My colleagues in the university world are horrified by my advocacy for this two-track system because they believe it will leave atelestic science without a figleaf of payback. I have more confidence in the Congress accurately reflecting the will of the people on this matter. Atelestic science may not, indeed, receive the most favored field treatment it has so far, if the Congress judges that the return on public investment would be much higher in telestic science and engineering. But one may also consider the possibility that the funding of atelestic science has been too high for 40 years relative to engineering, when compared to the actual desires of the public. And since we are concerned here with public funds, and a public that is going to be increasingly concerned with employment and the dire straits of our technical industries, that its representatives in Congress establish the most appropriate institutions to fund our science and technology operations. In this case I believe that will mean separate funding organizations for supporting National Technologies and for National Sciences.

Cooperation Not Competition in S and T

We have made many totally unsupportable statements that competition in the scientific funding system has been a major factor in the success of U.S. science. There is no way to prove or disprove such a proposition. Such huge sums being allocated to this activity under any structure is certain to produce more or less similar results. One notes that the British with much less open competition, and with smaller sums of money have been even more successful than we in Nobel prize science.

Again in sharp contradistinction to science, successful technology absolutely **demands** many modes of cooperation. Japanese culture reinforces this need for cooperation in technology. Here again we face the need to change our institutions so they tilt against instead of toward personal competition.

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Ms. Shapley?

STATEMENT OF DEBORAH SHAPLEY

Ms. **Shapley**. Thank you. Thank you very much for the chance to appear before the committee. It is not often that journalists are asked to do so and I am flattered by the request.

As you may already be aware, "Lost at the Frontier," the essay that Rusty and I wrote—of which we each have copies prominently displayed on this table, as he has already told you and as you already perhaps know from the committee's material—spends a certain amount of time castigating the science profession in the postwar years for putting forward what we argue is a myth: that basic science should be the sole centerpiece of a Federal science policy, and that if you fund basic research, technology applications, industrial competitiveness, prosperity—all of these things—will flow automatically.

I believe already this morning we have had a lot of discussion which bears on that key point. Our main point in the essay was that once you consider an alternate model—that is, once you consider that basic research does not automatically lead to innovation and you substitute a new paradigm, which is that much more complex linkages between science and applications between markets and capital are what produces cutting-edge advanced technology, then the making of U.S. policy for science and technology is a whole new ball game.

The goal of your Federal effort insofar as it affects the public consumers of the R&D and the goal of the Federal effort, and insofar as it is supposed to be a spur to private industry, in both of those dimensions you are talking about a rather different ball of wax.

Now, how does "Lost at the Frontier" deal with the issue before the Task Force today; that is, of research funding as an investment? I will briefly mention some of our recommendations that we present at the end of the essay and be glad to return to them in response to questions. I will then go on and make some comments about the overall circumstances of science and technology policy, which I think are very relevant to your inquiry.

First of all, we argue that we are not getting the most out of our investment in science and technology, that there is a lot of waste motion in the system and a lot of unnecessary duplication and disorganization. We argue that there is no single trail from the basic research laboratory to the marketplace. But that instead, we have to look at all the steps along the way and see how well the system is dealing with those. Take the case of manpower. Our essay is very critical of the turbulence in the present research funding system: the short lifetime of research projects; the numbers of grant proposals, which seem to have a Russian-roulette style of funding approval; the instability for not only tenured faculty as to where the next grant is coming from but of untenured talent and graduate students, has a cost in terms of friction within the system. And as we know, the demographic trends show that many of these untenured and junior personnel are fleeing the industry, in part, because of the relative security of careers. So in the manpower dimension, we would argue that we are not getting the most from our investment in science and technology.

We also argue that it is inefficient to encourage all American universities of any standing to try to be at the cutting end of basic research in all the major disciplines. That is slightly a caricature of the actual situation but in fact, Federal science policy for many years has encouraged such aspirations as a matter of professional pride among individuals, if not of explicit policy by the universities themselves. But it is well known that the most important research is done by a small subgroup of the science community anyhow, and that in addition, the costs of doing cutting edge science in many fields require facilities and instrumentation beyond the reach of many universities.

So we propose a change to university science ethos and a changed structure which would be to have the bulk of American universities emphasizing training and their contributions to future technical manpower and the conduct of applied science as a means of both giving research experience and contributing local and national needs. This would have the effect of consolidating some of the more ambitious research done within the present university system, although we explicitly state that we would leave areas of strength where they existed and try to have them be nuclei for the gathering of a more focused and targeted effort.

Now, as I say in my prepared text here, some of our reviewers have criticized this particular item as being too elitist. And we agree that it is elitist and we don't like to think that we are elitists, but we can't think of any other way to get the most efficient use out of the existing facilities that we have and to deal with the problem that you all have heard about: the vast costs of improving the existing basic research infrastructure in new universities.

There are other recommendations which address your issue of the research funding as an investment. For example, we believe that the science education picture, as Rustum Roy has said, is in drastic need of attention. And that this is again a structural issue that cuts across untraditional lines of science policy.

Our own particular recommendation was that there should be a change in the ethics in the profession of science in which most scientists donate pro bono time to the school systems and to the design, of course, of curricula. In other words, again, getting away from the "basic research as best" mentality. If, as a profession, there is a premium also placed on trying to help the national technology literacy, we might work nationally toward the goal of literacy that Rustum alluded to.

Then again, there are other suggestions which I will just mention briefly. One is a more strongly international effort. One way to get the most out of the investment in science is to get the most out of other countries' investment in science. And traditionally as a preeminent and rather insular power, the United States has not reached abroad as energetically to look at the research efforts of other countries. There are major exceptions, such as in the CERN high energy physics field. But in general, we don't go as much abroad to look at other countries' science as much as they come here to look at ours. This is something that could maximize our investment.

As Rustum has indicated, we are concerned also with the lack of orchestration among Federal applied science policy, and Rustum has alluded to the need for some kind of thrust in that area which would try to shift the balance away from what we consider a somewhat distorted professional value where basic research is considered the centerpiece of science policy and everything else is second class.

Now, just in closing, beginning on page 6 of my prepared statement, I talk about a final theme which is continuing my role as a science critic. These themes do not appear in the book as such but I think they are relevant to the question before you today.

It seems to me the great paradox of this situation that we are in, and that you are in, where we are all undertaking inquiries into what might be objectively the right way to go, redesigning a good science policy from the ground up, that during this period of widespread reexamination, the actual makings of U.S. science and technology policy should be in a highly ideological phase.

What makes science policy these days in the macro sense are not objective policies derived from careful inquiry such as the one you are undertaking, but what I would call—in order to stimulate discussion—shrill, unthinking political ideology.

I warn of this only because I believe it is going to be a real obstacle to the committee when it comes time to fashion recommendations and legislation and ask yourselves what political consensus you have in fact developed in the course of your hearings.

Let me explain what I mean by the political ideology of science support. What governs the gross characteristics of national science policy, particularly the Federal side of R&D today? In "Lost at the Frontier" we explain how Vannevar Bush fought the political ideology that prevailed in the executive branch and Congress before World War II—which was that the funding of science was none of Government's business and should be left to private philanthropy and perhaps the courts of Europe and the universities of Europe.

But after 1945, Bush and his colleagues undertook a change in political view. With their lobbying on Capitol Hill and the creation, subsequently, of the AEC, NIH—the expansion of NIH—the growth of defense research and the National Science Foundation as well, they brought into being a new political ideology which was, as you know, that it is the proper business of Government to support basic research.

You all know much more about that perhaps than we do so I won't dwell on that point, except to add that there was a corollary to the Vannevar Bush political ideology which has governed postwar science, which is that the Federal Government does not have a proper role in businesses' business. That is to say, the idea that the Federal role in science should be with basic research with no immediate application is very compatible with American political tradition, which says that the Government should stay out of things that belong to our private industry's sphere.

In our country we glorify the private market, the workings of competitive companies. And it has been fitting to American political traditions to have the Government stay out of the more applied end of civilian research. As you well know, there has been enormous growth in civilian research in a kind of Christmas-tree fashion; there has been constituent demand for food and drug regulation and there is a growth in research in that area; and there are demands for attacks on new diseases. So there are growths in research.

But I would describe that as a kind of Christmas-tree ornamentation on the overall system. I don't think that that grows out of any deeply-rooted belief that it is the Government's business to get into figuring how the semiconductor revolution should continue, for example.

So the corollary of an emphasis on basic research is a de-emphasis on civilian applied technology because, historically, that has not been the proper business of Government.

Very quickly, I will say what I think the non-rational political factors are that have guided research policy in the last few years. Again, these are macro factors and I don't think that they have been derived from close analysis of the actual research system.

Keyworth, in the President's first term, glorified the role of basic research while slashing back the Federal role in many applied civilian research and development programs. He advanced the postwar political ideology of basic research as best with a vengeance, where it neatly rationalized Dave Stockman's cuts in the larger and more politically vulnerable applied civilian programs, such as fossil fuels, and to some extent, fusion.

The irony is that while the applied programs became victims of Keyworthian rhetoric and Stockman's axe, more and more studies such as yours, have focused on the importance of applications as a link between basic science and innovation. But the prevailing political rhetoric has taken little account of these. I think we may pay a price for this political ideology. In effect, what has been promised is that this investment in basic research of the last five years, together with the private forces of the marketplace, will make U.S. industry competitive in the future.

I worry that the neglect of other problems—science education, engineering, the education of managers in industry—may well make George Keyworth's de facto promise fail us.

I mention two other political ideology questions which are shaping science policy whether we like it or not, or whether this committee individually would prefer it. This has to do with the growth of the Defense budget, which has been allowed to grow very rapidly, even as these civilian areas have been cut back.

Again, this derives from no rational or objective analysis of the needs of science and technology in the defense fields, nor from arguments that these defense increases will help civilian high technology competitiveness. Rather, the rhetoric is that it is an urgent task of Government to remedy weakness in national defense through a cornucopia of spending.

Star Wars is the most striking example of a science policy driven by political ideology rather than objective considerations. If the technical community, left to itself, without political cues, were to review the fields of physics, plasma physics, for example, and separately, computer software, and if their aim was to improve civilian high technology competitiveness, it is unlikely that they would choose Star Wars as the vehicle for achieving a better overall industrial health picture in the future. Nor would a science policy aimed at maximizing the health of all U.S. R&D necessarily take up a large portion of the Ph.D.'s who happen to be American citizens in classified research projects, and leave the work in civilian fields and the teaching of the next generation in some of these fields to foreigners.

Star Wars is exogenous to the overall purpose of the R&D effort which serves broader goals than a particular theory about ballistic missile defense. And yet, if Star Wars goes forward on the scale proposed, it will have a large and uncontrollable impact on the actual conduct of U.S. R&D.

Finally, I will make the same point about Gramm-Rudman-Hollings; namely, that if as is envisioned, civilian science and technology funded by the Federal Government continues to be held back as a result of Gramm-Rudman-Hollings, and if increases become increasingly difficult, again we may find the Department of Defense becoming the de facto maker of science policy because from the civilian side, they may turn to the Department of Defense to accomplish other goals.

In fact, we may see a reversal of the trend which began at the height of the Vietnam war with the Mansfield amendment in which civilian agencies were asked to shoulder the burden of general scientific support formerly carried by DOD. Now, in the future, after Gramm-Rudman-Hollings, impoverished civilian research programs may look to a comparatively richer DoD to underwrite the overall health and science. Already in the field of the university instrumentation, we see the Defense Department taking a large role.

My point is not that any of these prevailing ideologies are bad in and of themselves, or to argue them on their merits. My point is, that taken together, they do not add up to a science policy that is objectively derived to maximize the health of the overall effort, or the stated goal of Federal science policy—one goal of which is to help future high technology industry remain competitive in the years ahead.

Forty years ago, Vannevar Bush and his colleagues broke through the ideology of the day and the confusion in many minds at that time about what science was, how it should be governed, and the relationship of the priority given to science and to other Government priorities. This breakthrough occurred not only because Bush and his prestigious co-lobbyists made a good case, but because Congress was willing to go against the prevailing political ideologies which had governed decisions before.

One can only encourage and hope the committee to help make something similar happen today.

Thank you.

[The prepared statement of Ms. Shapley follows:]

Testimony of Deborah Shapley

before

Science Policy Task Force of the Committee on Science and Technology
U.S. House of Representatives

May 1, 1986

Political Ideology and the Support of Science

I would like to thank the members of the Committee and the task force for the opportunity to be here today. It is rare that journalists are invited to be Congressional witnesses, but perhaps from my three years as a writer for M.I.T.'s Technology Review, my eight years writing about the science policy scene for Science magazine, and my stint as Washington Editor of the British journal Nature, I can offer some observations. I have also published two books that deal with the role of science and scientists in our national community, the most recent being a study sponsored by the Ford Foundation, Carnegie Endowment and Resources of the Future, Inc., called The Seventh Continent, which deals with our scientific activity in Antarctica. I am here today in my capacity as co-author of with Rustum Roy, of another book: Lost at the Frontier: U.S. Science and Technology Policy Adrift, which some of you know about and to which Congressman George Brown was a most useful contributor. It presented a brief and controversial diagnosis of what ails American science and technology. Particularly, it outlined the difficulties we have under the present system of science support of getting the most return on investment in terms of innovation and competitiveness in civilian industry.

Rustum Roy has mentioned some of the main points of the book, but I would like to address, specifically, what the obstacles appear to be to getting maximum returns from the investment and the solutions Lost at the Frontier proposes. I would then like to discuss briefly a larger problem I believe your task force will have in drawing conclusions from these and other suggestions for reform and in promoting legislation. In the second part of my testimony I also offer a lay "critic" of science policy: it consists of some observations about political ideology of the support of science--a subject which is rarely addressed in detailed examinations such as yours but which, in my view, is a critical obstacle to any effort at reform.

I.

First, our diagnosis as it addresses the task force's current issue of research funding as an investment. We cannot answer your charge to be quantitative in this presentation, because Lost at the Frontier is very frankly an essay rather than a data-oriented study. We called it "an experiment in science criticism" and were gratified to see that some of our reviewers felt the essay called attention to important problems and made insightful suggestions. Our main point has already been signaled in Rustum Roy's testimony: it is that there is no objective proof that by investing in basic science alone, the nation will automatically grow fruits in the form of new technology and industrial competitiveness. As he said, our main point is derived from Vannevar Bush's quote, that science is only part of a "team" of which the other elements are: engineering, venture capital, markets, and a healthy and well organized applied science effort. Lost at the Frontier spends a certain amount of time castigating the science profession in the postwar years for putting forward the myth that "basic science is best" and preeminent in science policy--and the rest will take care of itself. We do see a trend away from this kind of propaganda today, as the spokesmen of science have had to admit that extracting innovation from basic research is a more complex and deliberate process. Nonetheless, the old mythology clings on and will be an important force in the outcome of the Committee's work, as I will discuss in the second part of my testimony.

Our main point was that once you consider that basic research does NOT lead automatically to innovation, and substitute a new paradigm--namely that complex linkages between science and applications, markets, capital and the like are what produces cutting edge advanced technology--then making policy for U.S. science and technology is a whole new ball game.

How does Lost at the Frontier deal with the issue before the task force today of research funding as an investment? To answer the question, I would like to call attention to two statements Chairman Fuqua made in his announcement of this hearing. "Should basic research be treated as a fundamental natural resource?" Fuqua asked. Lost at the Frontier's answer is yes, obviously, for as Rustum Roy just said, basic research provides benefits to all mankind: benefits to the spirit, to the quest for knowledge, and serendipitous economic benefits as well. But, as Chairman Fuqua also points out, "It is a long trail from the laboratory to the marketplace." This, we would argue, is the more important point.

Lost at the Frontier argues that there is no single trail "from" the basic research laboratory "to" the marketplace, that case histories of innovation--even of the atom bomb--show that a much more complex array of forces are at work yanking innovation out of the science. Thus, it is not enough, we argue, for federal science policy to only support basic research and leave the rest of the innovation process to take care of itself. Our ideas, then, fly in the face of the political ideology of the Reagan Administration's science policy as outlined below in Part II. Moreover, we argue innovation not only comes from science, for innovation also drives science, in the form of new instrumentation and new organization patterns for thought. It is all more complex than traditional science policy has assumed.

How does Lost at the Frontier address research funding as an investment? We believe several structural reforms are needed to properly maximized the investment we make in science.

A key one is manpower. Lost at the Frontier is very critical of the turbulence in the present research funding system, the short lifetime of research projects, the numbers of grant proposals and Russian-roulette practices for their approval, the instability of not only tenured faculty as to where the next grant is coming from but of untenured talent and graduate students--many of whom are fleeing to industry now in part because of the relative security of a career in industrial, rather than academic research. This problem we call the "human factor" which is imposing tremendous inefficiency and needless anxiety throughout the university part of our research system.

Lost at the Frontier also argues that it is inefficient to encourage all Amerian universities of standing to try to be at the cutting edge of their basic research fields. It is well known that the most important research is done by a small sub-group of the science community anyhow. In addition, the costs of doing cutting edge in science in many fields require facilities and instrumentation beyond the reach of many universities. So we propose a changed university science ethos--which is that their mission should be training and the conduct of applied science as a means of both training students and contributing to local and national technical needs. Such a change in goals for the university system as a whole would permit some pooling of resources among top ranked institutions for cutting edge basic research. Some of our reviewers have criticized the elitism of the proposal. It certainly flies in the face of the "Christmas tree" approach of recent years which attempts to spread around the benefits of high science facilities very widely. We do not like being elitists and recognize that it runs counter to the political traditions of science funding. But we see no other way to get the most benefit out of the investment in cutting edge basic research the nation has already made.

Lost at the Frontier also recommends many other structural reforms--such as a change in values of the professions of science and engineering that would permit professionals to become more engaged in science education. In the national debate over federal versus state and local responsibilities, we argue that another sector should play a role: the profession itself. Federal law, university practice, and professional society programs could encourage a quantum leap in the involvement of the profession in national technical literacy.

Other reforms we suggest include a stronger international effort, to get the most out of other countries' investment in R&D, particularly as Japan and Western Europe come abreast of us in fields we have traditionally dominated. We are also concerned as Rustum Roy said, with the lack of orchestration among federal and civilian applied programs and with their chronic inability to link usefully with industrial and consumer sectors they are meant to serve.

In short, Lost at the Frontier is equally concerned, as is this task force, with the overarching problems of how we invest in the natural resource of science and how users of the investment can get the most return from it. I would be glad to elaborate on our ideas further in response to questions.

II.

I now turn to my second and final theme, which is really an unsolicited comment on the valiant and in-depth effort the task force is making to arrive at some sort of objective and rational truth about what ails the U.S. science and technology enterprise and to reach reforms which can be agreed upon. I think it is a very laudible and ambitious effort you are making--far harder than the task we set ourselves in Lost at the Frontier. The essay, we insisted, should be no longer than 150 pages--it is 158 pages--and be capable of being read in a single airplane ride!

But the reforms that are needed--and our suggestions for structural reform of universities, industry, the oversight of federal applied science, of science education, and so on are but a sampling of the calls for reform you will hear --may well never be carried out for lack of a political consensus to punch them through.

It is paradoxical that during this period of widespread reexamination of the U.S. research enterprise that the actual formulation of U.S. science and technology policy should be in a highly ideological phase. What makes science policy these days is not the "objective" policies derived from careful inquiry such as yours, but what I would call shrill, and unthinking, political ideology. I warn of this only because it is going to be a real problem for the committee in fashioning legislation--not to mention an obstacle in getting good return on the national investment in science.

Let me explain what I mean by political ideology in science support. What governs the gross characteristics of national science policy, particularly the federal funding of R&D, today?

In Lost at the Frontier, we explained how Vannevar Bush fought the political ideology that prevailed in the Executive Branch and Congress before World War II, that funding of science (except for some military projects, and survey work such as the Geologic Survey) was none of the government's business. It was the job of private philanthropy and maybe of the courts of Europe. After 1945, Bush and his colleagues, through their report, Science, the Endless Frontier, and their lobbying, which led to the creation of the AEC, the expansion of NIH, the blossoming of peacetime defense research in universities, and the creation of the National Science Foundation, created a new political ideology for the support of science. As is well known, this was that the support of BASIC research was indeed a proper role of government. Though most accounts stress the fragility of this new agreement--and how slowly Congress in particular came around to voting regular appropriations of research funds--in fact the scientists' postwar lobby was successful beyond its early hopes. One result is the proliferation of research agencies and budgets we have today.

Now there was another part of early postwar political ideology for the support of science. A long-standing American political tradition is that government should stay out of the marketplace and permit the free market to operate. We all know that this isn't so: government affects business behavior in a myriad ways, through the tax system, patent policy, trade restrictions and the like. Nonetheless the notion that the federal government should stay out of the proper business of business is deeply rooted in our political psyche and has had important consequences for U.S. science policy. Thus, a corollary to the government-should-fund-basic-research axiom has been that government has no business sponsoring research which could conceivably be done in private industry: the perfect free market we imagine we have should take care of this. If some areas of research, applied research especially, are neglected by U.S. industry, then they should remain neglected--another example of the working of the free market, which drops "losing" efforts in favor of "winning" ones.

Thus, despite the enormous growth of research and development sponsored by the federal government, much of the applied and development work--particularly outside of defense and space--has been haphazard and poorly coordinated with industry. It is very much subject to political whims and the strength of lobby groups. This is precisely because the political ideology that governs U.S. science policy downgrades a federal role in this area--and we need only look to the federal program in mass transportation and housing for examples of the sorry results.

The irony is, therefore, that the largest part of the federal R&D effort has grown up in an unorchestrated way, often with very obtuse relationships to corresponding private sector efforts or private sector users of the research. Congress has played its role by responding to constituencies for particular diseases, for more or less regulation of food, or the environment. Thus it has permitted the bulk of the federal R&D effort to grow in a Christmas tree fashion, with more and more ornaments added and relatively few taken away. Thus, an added political understanding we have--though we do not like to talk of it this way--is that the R&D system can provide something for everyone. Though this results in disorganization and confusion, though it rarely solves the key problem of linking federal research to the private sector, the Christmas-tree approach is politically OK and even desireable.

The first Reagan Administration shaped science policy out of political ideology, in my view. These forces are still very powerful in our political life and may well have much more influence in setting U.S. science policy in the future than any objective policies your task force develops. Unfortunately, even as your task force makes a good faith effort to produce fresh recommendations, the federal science and technology effort is being shaped by political ideology, and non-rational factors exogeneous to science and technology and the objective health of the effort, more than ever. I don't know how your task force can deal with this, except to build the strongest possible political consensus for certain reforms and hope they can redirect the prevailing political winds much as Vannevar Bush's proposals changed political attitudes in his day.

Here are some examples of what I call non-rational political ideology, shaping the character of the federal S&T effort, and, it seems, hindering rational effort a reform:

(1) The rhetoric of George Keyworth, the President's science adviser in his first term. Keyworth glorified the role of basic research while slashing back the federal role in many applied civilian research and development programs. He advanced the postwar political ideology of science with a vengence, for it neatly rationalized David Stockman's cuts in the larger, more politically vulnerable, applied civilian programs such as fossil fuels and fusion research. The irony is that while the applied programs became victims of Keyworthian rhetoric and Stockman's axe, more and more outside studies have focused on the importance of applications as the link between basic science and innovation. The prevailing political rhetoric took no account of these studies however.

We will all pay the price for Keyworth's political ideology, I believe. In effect, he has promised that the investment in basic research, together with the private forces of the marketplace, will make U.S. industry competitive in high technology in the future. I worry that the neglect of other needs--strengthening science education, doing something about engineering, reforming the attitudes and education of management, for example--will make Keyworth's de facto promise fail us.

(2) A second example of the willingness of some politicians to treat research facilities as pure pork barrel and bypass normal technical review procedures. I am referring, of course, to the allocation of funds for specific research facilities on the floor of the Congress in recent sessions. These actions will erode whatever credibility and integrity the technical research system still has. Cornell, to its credit, bravely refused the funds on the grounds that they had been awarded in defiance of proper technical review procedures. These are extreme cases of the something-for-everyone, Christmas-tree approach to the funding of R&D. They do little to encourage hope for objectively fashioned, useful reform.

(3) Political ideology is determining the U.S. science and technology effort in another enormous way: the generous increases in defense applied research and development accounts of the last five years have been truly extraordinary, especially in light of Keyworth's and Stockman's cuts on the civilian side. Again, this derives from no rational or objective analysis of the needs in these applied fields, nor from arguments that these defense increases will help civilian high technology competitiveness which is part of Keyworth's science policy rhetoric. Rather the rhetoric has been that it is an urgent task of government to redress past "weakness" in U.S. national defense with a cornucopia of spending on hardware, manpower, infrastructure, on development and applied research.

Star Wars is the most striking example of a science policy driven by political ideology rather than objective considerations. If the technical community were to review the fields of physics, and, separately, where the best effort is needed in computer software, with the aim of improving the competitiveness of high technology civilian industry, it seems unlikely it would choose the Star Wars R&D program as the best vehicle. Nor would a science policy aimed a maximizing health of all U.S. R&D civilian and military, necessarily tie up a large part of the Ph.D.'s who happen to be American citizens in classified research projects, leaving the work in civilian fields--and the teaching of the next generation in some of these fields--to foreigners. Star Wars, it must be admitted, is exogenous to the overall purpose of the R&D effort, which serves broader goals than a particular--and unlikely--theory about how ballistic missiles can be shot down. Yet if it goes forward on the scale proposed, it will have large and uncontrollable impacts on the actual conduct of U.S. R&D.

One of the strangest developments is that the 6.1 money in the DOD research budget has not increased commensurate with DOD's applied research and development monies in the last several fiscal years. This would seem to contradict Keyworth's position on civilian R&D, namely, that increasing basic research is the key to future technology strength. Doesn't he want future technology to increase strength in defense as well?

I would also include the effects of the Gramm-Rudman-Hollings Act on my list of the way political ideology, rather than objectively derived criteria, are shaping the national science effort. Once curious effect of the act could be to place a much greater burden on the Department of Defense, whose budget (especially Star Wars) is protected from the government spending reductions the act mandates. As a result of Gramm-Rudman-Hollings, we may see a reversal of the trend which began at the height of the Vietnam War with the Mansfield Amendment, in which civilian agencies were asked to shoulder the burden of general scientific support formerly carried by DOD. Now, impoverished civilian research programs may look to a comparatively richer DOD to underwrite the overall health of science. Already in the field of university instrumentation, the Defense Department is taking a larger role. Most likely on other questions--such as the training of faculty, scholarships, and looking after neglected fields--DOD will be asked to play a larger role.

My point is not that any of these effects of the prevailing political ideologies are bad in and of themselves. My point is that taken together, they do not add up to a science policy that is objectively derived to maximize the health of the overall effort, basic and applied research, or to maximize the stated goal of federal science policy which is to help future high technology industry remain competitive in years ahead.

My point is that political ideology has increasingly determined the gross boundary conditions for U.S. science and technology. As the task force well knows, there are myriad problems--engineering a mess, early science education a victim of uncertainty between local, state and federal roles, many non-glamorous industries where patent activity still declines and which need attention but will get less of it because of Star Wars. But neither the ideology of the incumbent administration NOR that of the postwar period generally, steers us to a solution to them.

It is one thing for political leaders to favor a defense-comes-first position in voting the budget, but what does that mean for science policy? Does it mean that defense research should be budgeted and structured only for enhancement of national defense? Or does it mean that defense research should undertake a broader responsibility to assure "spinoffs" in the civilian sphere? It is the business of DARPA to look after the health of U.S. high technology industry? If this is DARPA's job, it is nowhere written down. My point is that past political ideologies for science are really no guide here: we are in a new forest and must blaze our own, fresh trails.

What is required is a new political consensus that can hack through the underbrush and navigate on the basis of present data and objectively derived needs, rather than with maps that have guided in the past, and make itself strong enough to take on the difficult work of structural reform.

40 years ago Vannevar Bush and his colleagues broke through the ideology of that day, and the confusion in many minds about what science was, how it should be governed, and the relationship of the priority given science to other government priorities. This breakthrough occurred not only because Bush and his prestigious co-lobbiests made a good case, but because the Congress was willing to go against the political ideologies which had prevailed before. One can only hope--and encourage the task force--to make something similar happen today. Thank you.

DISCUSSION

Mr. **Fuqua**. Thank you very much.

Ms. Shapley, since you have to leave, would greater Federal support to industry for basic research be more effective than direct participation by the Government in such research?

Ms. **Shapley**. You are asking what would be the relative effectiveness of more Federal basic research funds going to industry?

Mr. **Fuqua**. Yes, supporting industry to do it.

Ms. **Shapley**. The "Lost of the Frontier" position on that would say that any policy that encourages the cross-fertilization between industrial research and the rest of the research community is by definition welcome.

I didn't mention it and I guess Rustum only touched on it, but we are very concerned that industrial managers may not understand the role of basic research in their own enterprises despite these large increases in industrial R&D funding which are in and of themselves.

Mr. **Fuqua**. Is there any optimum relationship between the distribution of Federal support for basic research between industry and the academic community?

Ms. **Shapley**. I don't know how you would derive an optimum relationship. I think it would be very brave to try. I would think that the point would be to—

Mr. **Fuqua**. The point is that you don't see any measurement that you could——

Ms. **Shapley** [continuing]. Go by sector and target by industry as to where there are voids that need to be filled.

My point about the political climate for research funding is that perhaps we should not be timid in suggesting ways in which Federal policy can help those industries that are in difficulty.

Mr. **Fuqua**. Suppose you target an industry such as the semiconductor industry? I think it is generally recognized as one that is probably threatened the most from foreign competition and we need some draft choices to beef up our weaknesses on our team.

Here is a case where you have targeted an industry. I am not saying all industry, but here is one that the Federal Government made a mass infusion of research funds into industry to conduct this research.

Ms. **Shapley**. I would turn it to Rusty, but I will make one comment that bears on my last point with regard to the Defense Department making science policy.

Mr. **Fuqua**. Modern SDI, yes.

Ms. **Shapley**. But more to the point, the largest, as I understand it—I may be wrong about this—the largest Federal component of the—well, the most important Federal component of the semiconductor effort is through DARPA and associated programs in DOD. Now, DARPA, as I understand it, has no mandate to look after the competitiveness of civilian semiconductor industry. And there is an important Federal policy issue there, it seems to me, as to what the relationship should be between these competing goals. What's DARPA's interest in national defense—which is a perfectly proper one—and the use of that effort to help the competitiveness in the civilian's sphere?

Dr. Roy. May I answer that, Mr. Chairman?

In a sense, I think we are moving in the right direction already. In Chicago yesterday at the Annual Meeting of the American Ceramic Society, we met with the Fine Ceramics Association of the United States, which is a direct mimic of what the Japanese did in 1981.

Deborah said that anything we do to bring the communities together is good. I think any Federal policy is valuable which, with a relatively little bit of money can really form partnerships between Government, industry, and the universities. We are moving in the direction of not only just funding the research—that's the mistake—but also the joint guidance of that research. The role of the Congress is to create Government-university-industry teams that will facilitate this joint effort. The Nation must put in several centers—not a few—so that the research and training of students will be guided toward civilian consumer electronics and consumer computers—not only DOD stuff, which is not cost effective. I think that is a policy guideline which could guide a lot of policies—bring the communities together to plan, execute, and fund coherent programs.

Mr. Fuqua. Mr. Packard?

Mr. Packard. I don't have too many questions of either of these and I will leave it to the others more.

In DOD, Ms. Shapley, if DOD did not absorb a good deal of our research, do you think that we would transfer those resources to the research field outside of DOD, or do you think that——

Ms. Shapley. That we should or that we would?

Mr. Packard. That we would. I am looking now at the political will to do that. Much of the dollar targeting is done with political overtones. I think it would be foolish—at least for me—to try to convey anything otherwise; that political overtones often draw the dollars.

If DOD did not become that political vehicle to attract dollars to research areas, do you think that research would go hungry more for dollars?

Ms. Shapley. Congressman Packard, I think that is an excellent question and it goes to the heart of the choices that your committee has, as I see them.

It seems to me you have two strategies: one is that inevitably the national security argument in today's climate or in the next 4, 5, or 6 years, is the winning argument for the support of many activities which in another political climate might stand on their own. In that case, as I just alluded to in the case of DARPA, one needs to face it head-on and admit that, and recognize that the DOD money must also go to other purposes aside from the most obvious one of improving national security.

In other words, if one adopts the strategy that the DOD will be the drawing card for getting this money in the first place, then one should look very closely at the roles and missions and uses to which this is put, to see whether in addition it can be used to help civilian high technology trade in other goals which are subordinate.

The second strategy, it seems to me, is the one I alluded to in the last paragraph of my prepared statement and harks back to what

Vannevar Bush did in the 1940's, namely, we would argue, I think, that the case for a targeted research effort in the civilian sphere, in the absence of defense funding, would be very strong.

The problem is how to package that argument and to market it in the political sense so that were the consensus for defense spending to decline, one could have a substitute consensus saying we need to attack the problem of science and technology in the United States, and here's how to do it, and here are the chief arguments, and we want this money, and it is of great political urgency.

Mr. **Packard**. One last question of Dr. Roy.

In your zealousness to promote applied research, to move in that direction away from our commitment—at least in your judgment—our commitment to basic research and pure research, do you think that the pendulum could swing too far in the other direction?

Dr. **Roy**. No, I think in fact in your earlier statement you made a very interesting comment, that the Japanese took the route of building a technological base on which science could flourish. I am deathly concerned about the U.S. technological base. I believe that if we don't keep that firm there will be no science anyway—it will be an academic question because economic decline will be so severe if we do not pay attention to technology first. I challenge all my scientist colleagues with the question: "Who promised us a rose garden? Who promised us that we should be funded forever and ever—unless we pay some attention to that very same public?" So I believe exactly the opposite of conventional wisdom among university faculty that if we do not look to our technology base there will be no science. I believe the vast majority of scientists and engineers—most of them in industry—agree with me.

We can focus on our science base and there will be no technology and then there will be no science. So it is simply a matter of asking the question—in fact, which comes first, a healthy economy or the consumption good of atelestic science?

You see, I regard in economic terms most of the science that I call atelestic—as a consumption good—is not an investment at all. Your committee is talking investment strategy. But nobody thinks of a big art piece on the Chicago mall as an investment. It is in fact a consumption good. So a lot of our present basic science is a consumption good; not all of it. I call the latter part of basic science telestic—purposeful—that is long-range basic research connected to cancer, connected to materials making to civilian products.

I doubt if there is any danger of the pendulum swinging too far. No one is proposing that we cut it back in absolute money. The British have been through downward adjustments of several percent in such basic research I believe that even if we kept basic research steady, level funded, at present, and put all our new eggs into the technology basket, that would be a reasonable kind of approach. But if we don't rebalance the system—we are now so tilted towards atelestic research—we will be in trouble. If we continue with this tilt, it is not going to work. We have to retilt the system into some kind of balance between funding for technology and for science.

Mr. **Packard**. Thank you.

Mr. LUNDINE. Dr. Roy, you urge us to reinstitutionalize science and not just play an incremental game, and I gather that you would favor something like the creation of a technology foundation.

How does that fit with your appealing and vivid analogy of a football team? Wouldn't we be saying, "Well, basic research is over here in the National Science Foundation and applied science is over here in the technology foundation. "Wouldn't we be driving them further apart as opposed to trying to build linkages?

Dr. ROY. I am going to switch from football to my tree metaphor—the technology tree utilities science from all sources. The technology foundation as envisioned by Congressman Brown brought together the National Science Foundation's engineering directorate, it brought in Commerce, it brought in the Department of Energy—more or less all the civilian sector agencies and the civilian sector technologies. He brought together advisory committees from industry to guide the programs. This is precisely the kind of thing I am advocating.

Now, how does that connect to the basic research supported by NSF? It connects in precisely the way that the trees pick up the science from Britain. We pick up both science and personnel from the whole world. For example, all the electron microscopists in this country—they were all trained in England. So, I don't need to make them nor their science locally. Likewise, the national technology foundation and our industries will use the output of NSF science just as effectively, just as well as they are doing today. But there will be finally, an agency in the Nation—there will be an institution—to which you can say, "Hey, chaps, what are you doing in building the civilian infrastructure for a national technologically-oriented R&D program?" The relationship of that body to NSF basic sciences is that if somebody needs to use them—they will draw them in. There's nobody to draw in the output of NSF science today. So the linkages will be strengthened if we have somebody who is concerned about making the linkages.

Mr. LUNDINE. And you think it is necessary that the Federal Government prod industry to accomplish its fundamental mission?

Dr. ROY. I have been a consultant to many so-called high-tech industries for 25 years continuously. I meet with them monthly. They even pay me good money. Let me tell you, they are flat on their backs without effective utilization of basic research. I mean, I am not talking about anything that is going to happen, it has happened.

I am telling you that the General Electric—RCA merger is going to squeeze out now another big section of major industrial research. You look at the dozens of new industrial acquisitions, these paper entrepreneurships, and you find that they are killing American technology. Long-term research—essential to any technology—is simply dead in the United States. It is not in the future. It is here and now.

The U.S. industrial R&D infrastructure is on the ropes. So, I feel if the Feds don't intervene at this point—not by putting in big bucks, but in catalyzing it, in making it happen. I feel that that is a real institutional role which you must provide for now. Because without it, U.S. technological revitalization cannot happen. Given the reality of Wall street and its quarterly dividend, we cannot be rescued without Government action. J. Peter Grace keeps pointing to the incompatability between the quarterly dividend and modern technology. That is not only a Rustum Roy statement—even J. Peter Grace agrees.

Mr. LUNDINE. I am not sure that that is a recommendation.

Dr. ROY. It isn't in any other field. But when we find such diverse people agreeing on a fundamental issue, there may be some truth to it. [Laughter.]

Mr. LUNDINE. Ms. Shapley, I am fascinated by your observation that we are trying to allow too many universities to try to be at the cutting edge of science and that you raise the whole elitism question. Let me, as an aside, say that Mr. Packard and I had the opportunity—not this past weekend, but the one before—to meet with some of the executive officers of major American corporations and some university presidents. In one of these discussions, one of the major research universities in America got frightful if we adopted a policy, particularly with regard to capital investment—buildings and equipment—that would look to elitist.

I made the observation that I didn't know how they did business in the rest of the world but it seemed to me that what we would categorize as elitist would have somewhere between two dozen and four dozen universities still participating, whereas, in the whole rest of the world, my impression would be they might have one or two, or three, in major countries, or in all of Europe they may have four or five, but they wouldn't even have a dozen.

Would you tend to agree with that, and would you, therefore, in addressing the legitimate problems of instrumentation and capital facilities of universities, would you separate it out and say, frankly, whether through a—well, that's another question—but whether through a peer review process, or some other process, we are going to be, what in the American context would be elitist about the huge science investments, but at the same time, we are going to have a program so that equipment is relatively up-to-date at all the other institutions that would like to be at the cutting edge?

Ms. SHAPLEY. I think your problem was elitism in the American context is just a semantic one, because, obviously, the number in the elite is only a relative figure compared to the number drawn from. We have a much larger university system in this country than the Europeans do. I don't know what the numbers are, but the equivalent of three elite German universities in that population is an elite, whereas, in our larger system, 20 may count as an elite here. So, it may end up being comparable.

I think your underlying question is that—I mean, as to the university president's fear which you encountered two weekends ago, I think possibly what you were hearing is something that you would hear again and again and again if this committee were to formally propose a consolidation, for the same reasons, namely, that it flies against the grain of the American idea that every university ought to be able to struggle to better itself and be as much at the cutting edge as possible.

I will turn the rest of it over to Rustum, but let me stress something I said very quickly in my presentation—and it is here on page 152 and 153 of the book. We tried to be very clear in stating not that you take the top 20 ranked universities nationally and you give them all the bucks and the rest you forget about. And you say you are going to do applied research and we are not going to worry about you anymore.

What you do is you take—the universities themselves can do this, they can decide, we have got a nationally ranked chemistry department that is in the top 10 in the Nation, and we know that we have not been able to attract good physics talent for the last 20 years. We have been turned down by our leading five candidates the last searches we had. So let's be frank about this and put our resources into the chemistry department which is a nationally recognized part of the elite, if you will, and let's take that physics department and build it up to be strong in its own right, helping industry, helping other things that it can more legitimately do.

I call your attention to W.O. Baker's comment at the end of the book in which he makes—speaking from the Bell Lab standpoint—the point that if the university's primary mission is training, it doesn't necessarily follow that they have to train on certain kinds of cutting-edge frontier research.

What matters is that the research be of high quality and that the people be of high quality, and that the function be clearly thought out. So that one argument would be that a better mix of applied and basic science and a more consolidated basic research effort in fewer places could nonetheless train technical people just as well as the system does now.

Mr. **Lundine**. All right, accepting that—but can I throw another question in before you answer, Dr. Roy?

How would you make the decision as to where the investments go? Would you do it by some sort of peer review? Would you let Congress do it, in which case it will be some in his district?

Ms. **Shapley**. You have to let the community decide, I would think.

Mr. **Lundine**. The scientific community.

Yes, or the university populations. A lot of this may happen anyway, just because of the pressures of economics. But I would be strongly opposed to any kind of politicization of those decisions, because they are, after all, technical ones.

Mr. **Packard**. Would the gentleman yield for a moment on that point?

Mr. **Lundine**. Certainly.

Mr. **Packard**. What you are suggesting perhaps then, is what's, to some extent, happened in the medical profession where everyone tends toward a specialty.

I am wondering in science if that would then become a serious problem of funding because there are some specialties in research that really are not attractive in terms of attracting funds, or attracting some of your high quality minds. And that brings up the question of being number one our fundamental goal in every field and in every area. Certainly that would promote that concept that a university, if it is not number one or certainly in the elite group, in chemistry or in biomedical research, or in other ag programs, or whatever, then do we find ourselves losing sight of the broad picture? And, again, the pendelum swinging too far in the other direction?

Dr. Roy. I think I want to go back to Congressman Lundine's question and then come back to yours, Congressman Packard, because Congressman Lundine's number was just about right—20 to 25. If you look at Britain, they have got basically Cambridge, Oxford, and Imperial College—they have made that decision. They have bitten the policy bullet.

We in the United States aren't willing to bite any policy bullets. We simply have to say to many universities, "Everybody can't be No. 1 and, therefore, you can't pretend to be No. 1." But we are not going to decide by universities—we propose to choose and mix at the departmental or lab level. You would have a heterogeneous mix on the campus of national centers of excellence in departments. Furthermore, I would hope the hierarchy of deciding what's No. 1 also changes. The State role in funding is now becoming very important and the more applied groups may be recognized for their contributions to the State's economy. We have the Morrill Act, and we have the Hatch Act. We have before you models of saying, if we could do in engineering what the Hatch Act and Morrell Act have done in agriculture—to essentially couple the State money to match the Federal money to make the university a resource for industry in applied science and engineering. That is the model I look for.

The nationally recognized groups could appear on any campus. A particular campus may have three such engineering groups and one science group; another may have one. So we would not make elitist universities, although some universities would obviously have a concentration of these—but we would make specialized departments. There are too many universities which are merely clones of each other. There is too little diversity on the campus. They are all too homogeneous. So I suspect that one of the outputs of this new initiative would be to create a new set of more heterogeneous universities, some very good in a few scientific fields, others very good in a few technological fields. There would be a much healthier mix, in our view.

Mr. Lundine. And you would have sort of two levels of programs: One, there would have to be some kind of grants to available to those universities that weren't world class in chemistry so that they have adequate chemistry labs.

Dr. Roy. Certainly, and that could be decided by the peer-reviewed formula which we went through last time I testified. I have shown how terribly wasteful this whole system of mail-proposal peer review is. Peer review is essential; the community makes the decisions on quality. But I have developed at great length a productivity-based formula, which, by the way, nobody has challenged yet, for the disbursal of Federal funds. One of the agencies has said it's pretty good. But nobody has bothered to give it to their staff and say: "What would happen to our funding if we switched to Roy's formula?"

I believe productivity, peer-reviewed, is an essential parameter in funding decisions. Promises, peer-reviewed or not, are worthless. So I believe in peer review, but the peer review of production, not of promise.

I think my colleague is going to depart.

Mr. Lundine. Yes; thank you very much.

Mr. **Packard**. Thank you so much.

Do you want me to chair?

Mr. **Lundine**. Yes.

Mr. **Packard** [presiding]. Do you have any other questions, Mr. Lundine?

Mr. **Lundine**. No; I don't

Mr. **Packard**. Nor do I, Dr. Roy. We sincerely appreciate your appearance again here. You certainly stimulate our thinking and we appreciate it very much.

Dr. **Roy**. Thank you.

[Question and answer for the record follows:]

SCIENCE POLICY TASK FORCE

Hearings on Research Funding as an Investment

May 1, 1986

Followup Question for Dr. Roy

Your commentary on present research policy was indeed thought provoking. However, we saw no statement addressing the essence of this particular hearing, viz., how do we measure return on an investment in basic research? Please provide additional comment on this point.

My testimony did in fact refer to your specific question. On p. 5, first sentence, my opinion is given, viz:

The OTA study combined with the documents from the University of Sussex, more than adequately summarize the attempts at quantifying the ROI from research, and that such studies are very elusive, at best.

But it is a common error (made by Lord Kelvin and many lesser successors!) to assume that what cannot be quantified cannot be judged, evaluated, etc. The rest of my testimony is my answer to your question. We can measure ROI in basic research:

- o First, decide the sign of the effect negative or positive.
- o Second, estimate qualitatively the factors which affect positively or negatively the ROI.

This is what I attempted to do in my text. On re-reading my own text it is obvious that I failed to convey the logic of my approach. Hence your inquiry was very appropriate.

Mr. **Packard**. We are pleased now to welcome to the hearing Dr. Mary Ellen Mogee. We sincerely appreciate you also being with us, Ms. Mogee.

Ms. Mogee is from Great Falls, VA; consultant in science and technology policy. You may wish to either present your testimony as written or you may wish to summarize, as you choose, and rest assured your written testimony will all be entered into the record.

[A biographical sketch of Dr. Mogee follows:]

Mary Ellen Mogee, Ph.D.

Dr. Mogee is a policy analyst specializing in industrial innovation, technology transfer, science and technology indicators, and international competitiveness. She is currently an independent consultant, located in Great Falls, VA. Previously she held positions at the National Science Foundation, Congressional Research Service, National Bureau of Standards, the Patent and Trademark Office, and the Business-Higher Education Forum. She received her bachelor's degree in chemistry from MacMurray College in Jacksonville, IL, her master's degree in science, technology, and public policy from the George Washington University and her doctorate in political science from the George Washington University. She is the author of numerous reports and publications. Among those relevant to her topic today are "The Relationship of Federal Support of Basic Research in Universities to Industrial Innovational and Productivity," in U.S. Congress, Joint Economic Committee, *Special Study on Economic Change* Volume 3 *Research and Innovation*, 96th Congress, 2d session, 1981; *Technology and Trade: Some Indicators of the State of U.S. Industrial Innovation*. U.S. Congress, House Committee on Ways and Means, Subcommittee on Trade, 96th Congress, 2d session, 1980; and "Patents and Technology Forecasting," in *1987 Yearbook of Science and the Future*. Chicago: Encyclopaedia Britannica, forthcoming.

STATEMENT OF DR. MARY ELLEN MOGEE, CONSULTANT IN SCIENCE AND TECHNOLOGY POLICY, GREAT FALLS, VA

Dr. **Mogee**. I will summarize my remarks.

I am pleased to be here today to share with you my thoughts on the feasibility of using patent data to assess federally funded research. I find that it fits right in with the other presentations that have been given, because I will be talking about discovering the links between science and technology.

I will be talking mostly about basic research in universities, but some of the ideas may also apply to research done in Federal labs.

I would like to make three points.

No. 1, patents are a rich source of data for science and technology policy analysis. I have included a copy of a patent as an appendix to my statement primarily to illustrate this point. I won't go through the various data items that are included there for legal purposes, but are of great interest to people who are interested in science and technology policy. I would like just to make the point that advances in information technology have made this kind of data much more accessible and useful to analysts.

My second point is that patents are more limited as a source of data about outputs of basic research and especially basic research in universities than they are for science and technology activities that are closer to the development end of the spectrum and that occur in industrial settings.

My third point, though, is that it is nonetheless possible to design studies using patent data to address questions dealing with the output and impact of basic research—and this has been witnessed by a small number of studies that have been conducted in the United States and other studies that are underway in Great Britain, Australia, and New Zealand.

Patent data are one of the few available indicators of the output of R&D, as opposed to the inputs—such as R&D funding and manpower. Moreover, patents are screened for novelty, utility, nonobviousness, and complete disclosure by the Patent and Trademark Office.

Patent data also have a number of problems as indicators of scientific and technical activity. Among these are the fact that not all inventions are patented, patents are not of equal technical or economic significance, and the propensity to patent varies across industries and technologies.

My feeling is that patent data are like a tool—we need to learn how to use them for policy analysis. My metaphor is that like a child who has been given a hammer, first we hammer everything in sight, and only through experience do we learn which things respond well to hammering and which things don't.

I would like to suggest that we think a minute about basic research and the universities in which it is conducted, and we will probably agree that patents are not as suitable for assessing the output of research as they are for other kinds of scientific and technical activities. Whereas patents imply utility and commercial potential, basic research findings are often many steps removed from practical applications and economic return.

Moreover, in universities, where about half the basic research in this country is done, patents do not reflect a primary institutional objective. Although the generation and dissemination of scientific knowledge are purposes of universities, the main means of dissemination is through the education of students and publication in the open literature.

Universities tend to value the free and wide distribution of knowledge while patents place restrictions for a limited period of time on who can use the invention. This is done in order to give the inventor incentive to commercialize his or her invention.

The low priority given to patents by universities is reflected in their patenting levels. Table 1 in my paper shows patenting by the top 10 universities as ranked by the number of patents assigned to them from 1969 to 1984. And then for comparison purposes in table 2, the top 10 corporations are ranked by the patents assigned to them in 1984. I am not going to dwell on these numbers at any great length. The main point I wanted to make is that university patenting is relatively low. If you look at the total number of patents to universities in 1984, it was 520. That is about 2 percent of the number of patents assigned to U.S. corporations in 1984, which is close to 28,000.

And if you look at the number of patents assigned to the top universities—47 patents in 1984 to MIT and 45 to the University of California system—those numbers are much smaller than the numbers assigned to General Electric at 785, and IBM at 608, the same year.

Now, my point is not to denigrate the importance of universities as a source of scientific knowledge, but really to show that patents have not been traditionally important to universities as a means for disseminating that knowledge.

There is some evidence that this situation is changing as more universities are setting up patent offices and trying to increase revenues to the university through patent licensing. Some universities have achieved relatively high visibility through licensing of important patents, and the Cohen-Boyer genetic engineering patent at Stanford comes to mind. It is still too early to tell how successful those kinds of efforts will be.

But the main point I wish to draw is that because relatively few patents come out of university research, they are limited in utility in assessing that research. I mean, you shouldn't take an activity that is relatively unimportant and use that as a primary indicator of the output of research.

But I will go on to say that limited utility does not mean complete lack of utility, and I would like to review some studies that have been done using patent data to address questions dealing with the output and impact of basic research.

The studies fall into two groups. The first is work that has been done at Computer Horizons, Inc., a private firm in Cherry Hill, NJ, which applies bibliometric methods to the citations in patents in order to trace linkages between the technology as revealed in the patents and scientific advances in the scientific literature.

The first study was of two technologies—prostaglandins and gas lasers. They found that a large share of the citations in those patents was to journals, and that the majority of those journal citations was to basic and applied research journals rather than to engineering and technology journals.

I will mention parenthetically that a large share of the citations were also to earlier U.S. patents. But a significant share of them were to the scientific literature.

The articles cited were relatively recent. And by looking at acknowledgements of financial support in the cited articles, the researchers were able to find that the Government was a source of financial support in 25 to 30 percent of the cases.

So this shows to me that at least in two fields of technology, technological developments as revealed in patents draw heavily on scientific advances and that the Federal Government is a source of financial support for a significant proportion of the scientific advances on which technology draws.

Now, in my written remarks I speculate about some ways in which this particular line of research could be continued.

Computer Horizons performed a validating study wherein they compared the patent citation rankings of the "science dependence" of 24 technologies, and found that it agreed closely with expert rankings of science dependence.

Mr. PACKARD. Dr. Mogee, would you accept my apologies for interrupting, but we are going to have to recess long enough to go vote and then we will come back. We have got about 5 minutes before the voting period is over, so could we just hold it in recess and then we will return, hopefully, within the next 10 minutes to resume.

Dr. MOGEE. That's fine.

[Recess.]

Mr. FUQUA [presiding]. We will resume our hearing.

Dr. **Mogee**. The two rankings were found to have a high rate of agreement. Those technologies ranked by experts as highly science-dependent had a much higher average number of citations to journals per patent than did the technologies ranked by experts as having low science dependence. Testing for correlation in rank-ordering with Kendall's tau, they found that the number of references per patent to journals correlated highly with the expert judgments.

This type of research could be expanded to additional technologies. If Congress is interested in whether U.S. science is contributing to fields of technology that are particularly important to national welfare, defense, or international competitiveness, the method has potential for assessing the degree to which federally funded research is contributing to these technologies. Designed differently, a study could use the patent citation methodology to review the areas of science that are drawn upon in current patented technology developments.

The question these studies do not answer is whether science is contributing as much as it could or should to technology and how to change Federal support for research to increase that contribution, if an increase is deemed desirable.

While it might be tempting to try to reallocate funding in favor of areas of science that contribute to technological developments in important areas, it must be recalled that many of the citations were to basic research areas not directly relevant to the area of research. One area of research does not proceed in isolation from others, and advances in one may be delayed by lack of progress in others.

We currently have an inadequate understanding of the nature of research and the interconnections among fields of research and the links of science to technology to guide policies that fund certain areas of science in the hope of near-term practical applications. Patent data can add somewhat to our understanding, but they alone are insufficient.

Studies of patents resulting from NSF research grants. The National Science Foundation has supported two studies attempting to determine the extent to which NSF grants led to patented technology and to estimate the economic value of those patents.

The first study was of the NSF chemistry program. The names of NSF principal investigators were matched with the PTO's computer list of inventor names on U.S. chemistry patents. And this is done using a computerized file at the Patent and Trademark Office. Once the patents have been identified, it was then determined whether the patent actually resulted from the NSF grant.

The study of chemistry grants compared NSF results with those of the Research Corp., which is a private foundation. Both showed that about one chemistry PI in 100 produced patents related to the grant.

The study of engineering research project grants found that about 20 percent of the PI's produced patents related to their NSF grants.

Thus, if you compare those two, you find that engineering research PI's were 20 times as likely as chemistry research PI's to produce patents from their grants.

Now, this kind of comparison is useful, I think, for what it tells us about the nature of research in different disciplines and its connection to practical applications. I do not feel that they would justify a conclusion that engineering research is somehow more productive than chemistry research, because they are producing different things.

I will go on to describe these studies a bit more. About 15 percent of the related patents in both studies were licensed. And the studies then attempted to estimate the economic value of the patents on the basis of licensing income. Now, this is highly problematic for a number of reasons, including difficulty of obtaining data for patents that are licensed; the fact that some patents that may eventually be licensed were not yet licensed at the time of the study; and the extreme uncertainty of estimating future licensing income.

Moreover, I would just like to say something about the distribution of economic value across patents. It is widely accepted that many patents do not have high economic value, and that the patent that earns millions of dollars in royalties is the rare exception. This in fact turned out to be the case in the NSF study of engineering research. They found that, I think it was, the seven patents that were licensed earned $680,000 in royalties, but $620,000 of that came from one patent.

So that the high value of one patent can swamp and distort the statistics on licensing income.

There are other reasons to believe that licensing income is not a good measure of the economic value of university patents. The university may be constrained in negotiating with potential licensees because it lacks the pervasive profit maximizing motivation that companies have. It often lacks a broad portfolio of related patents and know-how that a firm would bring to the bargaining table; and the university often depends on industrial companies for funding, for gifts, and for employment of graduates, and hence, may hesitate to push for high royalties.

Table 3 in my written statement shows licensing income from what are probably the most actively licensing universities. I regard these figures as fairly small. It is interesting that they all cluster around $2 million a year, which suggests to me that this somehow is a cap that is imposed more by administrative considerations and the aggressiveness of the licensing activity as opposed to the scientific and technical activity in those universities, especially because there are differences in sizes among the universities.

University patent administrators freely admit that maximizing license income is not their primary objection, but rather, technology transfer, or using patents to attract additional research funding from industry.

For these reasons, I would recommend against using licensing income as a measure of the economic value of university research.

On the other hand, the use of patents to trace the movement of advances in scientific knowledge into industrial applications has merit, although it must be recognized that patents don't represent all the impacts that research may have on technology.

The method should probably not be used to compare disciplines for making funding decisions, but it seems appropriate for evaluating progress over time in programs that are intended to encourage the transfer of basic research results into industry. Thus, it appears to me that the method has potential applicability to assessing the progress of the NSF engineering research centers, the University-Small Business Patent Amendments of 1980 and the technology transfer functions of Federal laboratories.

As a closing note, I would like to mention that the Office of Technology Assessment and Forecast in the Patent and Trademark Office in the past provided much of the patent data that are used in science indicators and in other science and technology policy analysis. But that office was terminated last year as a budget measure.

This action seems particularly shortsighted to me in view of the concerns of this Task Force with measuring the output of the Nation's science and technology. I would, therefore, appreciate your support in seeking the reestablishment of that office so that it could continue to provide patent data and analysis to Government policymakers and to the public.

Thank you.

[The prepared statement of Dr. Mogee follows:]

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THE FEASIBILITY OF USING PATENT DATA TO EVALUATE FEDERALLY FUNDED RESEARCH

Statement of Mary Ellen Mogee, Ph.D.,
Consultant in Science and Technology Policy

Before the Science Policy Task Force
May 1, 1986

I am pleased to address you today on the feasibility of using patent data to evaluate federally funded research. My remarks pertain primarily to federally funded basic research in universities.

I am speaking to you as an independent analyst who has conducted research and analysis on issues of science and technology policy in the federal government for the last 15 years. I have had the opportunity to work the broad spectrum of analytical activity, from that which is directly policy relevant, such as evaluation of proposed legislation at the Congressional Research Service, to that which is more methodological, such as generating patent data at the Patent and Trademark Office and resolving issues on how that data should be used. I approach my task today by trying to integrate the nature of patent data and the kinds of policy questions about federally funded research that patent data can address.

Perhaps the first thing any person who is advocating the use of patent data needs to do is acknowledge the criticisms that have been aimed at patent data as indicators of technological activity or economic value. I have reviewed these criticisms in another paper and will not dwell on them here.[1] Over the last 15 years or so, much effort has gone into examining the adequacy of patent data for describing and measuring technological change. The feasibility of using patent data to answer a variety of questions has been explored, as have problems associated with use for various purposes, and ways of overcoming those problems. I liken this process to that of a child who has been given a hammer. First he decides that everything needs hammering and only through experience learns which things respond well to hammering and which things should not be hammered. Although still in the midst of this process, researchers and analysts have gained a better understanding of the sources and magnitudes of bias and possible misinterpretation, the circumstances in which patent data are valid indicators and those in which they are not, and the kinds of data that are useful complements to patent data.

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[1] Mary Ellen Mogee, "Patents and Technology Forecasting," in 1987 Yearbook of Science and the Future, Chicago: Encyclopaedia Britannica, forthcoming.

This leads to the three main points that I would like to make today:

1. Patents are a rich source of data potentially useful in addressing a variety of science and technology policy issues.
2. Patents are more limited as a source of data about the outputs of basic research, especially basic research in universities, than for scientific and technological (S&T) activities closer to the development end of the spectrum and those in industrial settings.
3. Nonetheless it is possible to design studies using patent data to address questions dealing with the output and impact of basic research, as witnessed in a small number of studies conducted in the United States, Great Britain, and Australia.

I. Patents are a rich source of data for science and technology policy analysis.

I have included a copy of a U.S. patent as an appendix to my testimony to show the wealth of information contained. Some of the useful data in a patent are: name of inventor and address, name of owner (assignee) at time of patent issue and address, type of technology (classified into 400 classes and 112,000 subclasses), references to prior art, that is, the closest related science or technology; dates of application and issue, description of the invention, and legal claims. Advances in information technology have made this data much more accessible and usable for policy analysis purposes.

Patent data have a number of advantages for S&T policy analysis. One of the most important is that they are one of the few available indicators of the output of S&T activities. They are objectively screened for novelty, utility, nonobviousness, and complete disclosure. They cover a broad range of technologies, many years, and many countries.

There are basically two ways in which patent data may be used for S&T policy analysis:

1. Counts of standard data items captured on electronic patent data bases, followed by statistical analysis. Keith Pavitt of the Science Policy Research Unit of the University of Sussex has distinguished three general analytical approaches to patent statistics in the context of science and technology policy:
   - monitoring trends in patenting at fairly aggregate levels, such as in Science Indicators
   - using patent citations and co-citations to trace relationships among fields of technology, between science and technology, and between technological activity in different countries
   - using patent statistics to explore causal relationships between patenting activity and other measured economic variables to test theories and models about determinants and consequences of

technological activity, such as the relationship between R&D, patenting, and productivity.[2]

2. Analysis of the technical information contained in the patents. This generally requires technical expertise. An example of this is use of patents in a literature search by a corporate researcher looking for a solution to a particular technical problem.

Both of these approaches have been applied to study the output of research.

II. Patents are more limited as a source of data about the outputs of basic research, especially basic research in universities, than for scientific and technological (S&T) activities closer to the development end of the spectrum and those in industrial settings.

A. Basic Research and Patenting. In some respects there is a contradiction between the conditions associated with basic research and those associated with patenting. To be patentable, an invention must have utility. Applying for a patent generally means that potential commercial value is seen in the invention. Basic research, on the other hand, is undertaken for the purpose of adding to knowledge, without any practical application in mind. Significant advances in scientific knowledge are often not directly and immediately applicable to the solution of practical problems and are many steps removed from economic return. The idea must be further developed and applications explored. The necessary supporting technologies for producing and using the invention must be adequately developed. There must be sufficient economic advantage over competing economic goods to ensure a market.

The output of basic research has been likened to a pool of scientific knowledge that is available to all parties to draw upon for whatever purpose.[3] The use of this knowledge is often indirect and delayed, and may take as much as 20 to 30 years. The results of basic research can be transferred into applications very quickly in emergencies and in science-based industries such as biotechnology, chemicals, and electronics, but these situations represent the exceptions rather than the rule.

B. Universities and Patenting. Scientific activity and patenting cannot be understood outside of the context within which they occur.

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[2] The uses of patent statistics in science and technology policy analysis are reviewed by Keith L.R. Pavitt, in "Patent Statistics as Indicators of Innovative Activities: Possibilities and Problems," Scientometrics, v. 7, January 1985: 77-99.

[3] Mary Ellen Mogee, "The Relationship of Federal Support of Basic Research in Universities to Industrial Innovation and Productivity," in U.S. Congress, Joint Economic Committee, Special Study on Economic Change, volume 3, Research and Innovation, 96th Congress, 2d session. Washington, D.C.: U.S. Govt. Print. Off., 1981.

Patents are useful to study scientific and technical activities in industry because patents reflect industry's primary motivation, which is to make profits. In industry, S&T activity is undertaken for the purpose of generating new products for sale or for improving production processes to reduce cost or improve product quality. A patent application in this context means that an invention is perceived to have commercial potential that is significant enough that the company wants to have proprietary rights to that invention.

About half of the basic research in the United States, however, is performed in universities. In the university, patents do not reflect a primary objective of the institution. The main purposes of the university are:

- to discover knowledge
- to preserve knowledge
- to disseminate knowledge

The main means of disseminating knowledge other than through the education of students is through publication of books and journal articles. The maxim in academia is "publish or perish." The real incentive is to publish in the open literature, because position and tenure depend on it. This institutional incentive is reinforced by professional standards that encourage publication.

In fact, there is a certain amount of conflict between the values of publication and patenting. Publication makes knowledge freely available to anybody who is interested or able to use it, thus contributing to its widespread dissemination. On the other hand, since nobody "owns" the knowledge and thus cannot prevent others from using it, such knowledge is not an attractive base on which to build a business because others with the same knowledge are likely to compete, driving down one's profits. Patenting, while it also results in the publication of the invention, gives the inventor property rights in the invention for a limited period of time. This is intended to give the inventor an incentive to commercialize the invention by protecting him from competition to some extent. The argument that has not been settled is whether publication or patenting leads to more social benefits from the knowledge. To date, universities have decided on the publication route.

For these reasons patenting has a relatively low place in the hierarchy of values in universities. This is reflected in the patenting levels of universities. Table 1 shows the top 10 U.S. universities as ranked by the number of U.S. patents issued 1969-1984, plus Research Corporation and University Patents, Inc, two organizations that help universities commercialize their patents. Table 2 shows, for comparison, the number of patents issued to the top 10 corporate assignees in 1984.

TABLE 1

TOP TEN UNIVERSITY PATENTEES -- 1969-1984

University	Number of U.S. Patents Assigned 1969-1984	1984
Massachusetts Inst. of Technology	692	47
University of California	385	45
California Institute of Technology	240	15
Wisconsin Alumni Research Foundation	230	16
Iowa State Research Foundation, Inc.	214	14
Stanford University	163	36
Purdue Research Foundation	140	14
University of Illinois Foundation	130	6
University of Utah	113	8
Johns Hopkins University	98	10
*Research Corporation	534	31
*University Patents, Inc.	116	25

Notes:
* = Patent commercialization organizations that specialize in commercializing university patents.
The total number of U.S. patents assigned to U.S. universities in 1984 was 520.
Data source: U.S. Patent and Trademark Office.

TABLE 2

TOP TEN CORPORATE PATENTEES -- 1984

Company	Number of U.S.Patents Assigned in 1984
General Electric Company	785
IBM Corporation	608
Hitachi Ltd.	596
Toshiba	539
U.S. Philips Corp.	438
Canon	430
RCA Corp.	428
Siemens Aktiengesellschaft	405
Mobil Oil Corp.	373
Nissan Motor Co.	372

Notes:
Source of data: U.S. Patent and Trademark Office. The total number of patents assigned to U.S. corporate patentees (primarily corporations) was $27,988 in 1984.

The main point I wish to make with these tables is that the number of patents assigned to universities is relatively small. The total number of U.S. patents assigned to U.S. universities in 1984 was 520, about 2 percent of the total number of U.S. patents assigned to U.S. corporate patentees that year, which was $27,988. Moreover, the number of patents assigned to the universities with the largest number of patents, MIT with 47 and the University of California with 45, is very small compared to those of the largest corporations, General Electric with 785 and IBM with 608. The number of patents falls off rapidly as we go down the list of universities. This means that most universities handle a small number of patents. In a situation like this it often does not make sense to have an office devoted to obtaining and commercializing patents. The lack of an organized, aggressive commercialization program may in turn decrease the economic value the university receives for its patents.

The intent of this comparison is not to denigrate the importance of universities as sources of scientific and technical knowledge, because they are very important. Also, there is evidence that universities and university researchers are becoming more patent-conscious. The University - Small Business Patent Amendments of 1980 (P.L.96-517) allowed universities to take ownership of patents resulting from government sponsored research. More schools are setting up patent offices. Some of them have achieved high public visibility through the licensing of important patents such as the Cohen-Boyer genetic engineering patent at Stanford University. It is probably too early, however, to tell what the overall effects of these changes will be on the universities.

The point here is that patents traditionally have not been an important vehicle by which universities disseminate the knowledge they generate and that most universities are probably not set up to maximize patent revenues. Therefore patents are limited in utility for studying the output and economic value of university research. Being of limited utility does not mean that they lack utility completely, however. It means that care must be exercised in the kinds of studies that are done using patent data and the conclusions that are drawn from them.

III. Studies can be designed using patent data to address questions dealing with the output and impact of basic research.

Although not much work has been done in this area, a few studies have been completed in the United States using patent data to assess the output or impact of research. Efforts have also begun in Great Britain, Australia, and New Zealand to use patent data to assess national S&T capabilities. I am not aware of any results from these efforts to date, but it is significant that other countries see potential value in patent data as analytical tools for science policy.

The studies that have been done in the United States fall into two groups, representing two different methodologies and addressing different questions:

- patent citation studies of links between technology and science
- studies of patents resulting from programs of research project support

A. Patent Citation Studies of Links Between Science and Technology. As described earlier, patents contain citations to the "prior art," that is, the closely related preexisting science and technology. Researchers at CHI Research/Computer Horizons, Inc., (CHI) have been applying some of the methods used in bibliometrics to the citations in patents in order to trace linkages between technology and science. The principal assumption is that patent citations to scientific literature signify a link between the scientific knowledge in the article and the invention described in the patent.

An early study covered two technology fields -- gas lasers and prostaglandins.[4] Patents in these technologies were identified and the citations in the patents were extracted. A large share of the citations was to articles in journals (one-half of the citations in prostaglandin patents and one-third of the citations in gas laser patents). U.S. patents also accounted for a large share of the citations.

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[4] Mark P. Carpenter, Martin Cooper, and Francis Narin, "Linkage Between Basic Research Literature and Patents," Research Management, v. 23 (March 1980): 30-35.

The patents cited research articles quite heavily. In prostaglandins, 80 percent of the journal citations were to basic research journals and 13 percent to applied research journals. In gas lasers 33 percent of the journal citations were to basic research journals and 56 percent to applied research journals. Moreover, the articles being cited were recent. Prostaglandin patents cited literature only 2-3 years older than that cited in chemical papers, while gas laser patents cited papers that were more recent than those cited in physics journals. While the patented inventions came primarily out of industrial laboratories, they drew heavily on recent research findings.

By looking at the acknowledgments of financial support in the cited articles, the researchers found that in prostaglandins the federal government was the source in about 30 percent of the cases. The NIH was the source in about 25 percent of the cases and the NSF in about 3 percent of the cases. The 8:1 ratio of support of cited papers supported by NIH and NSF in the prostaglandin patents is close to the overall ratio of NIH/NSF support in the basic and applied research biomedical research literatures, which is known from previous research to be about 7:1. Thus NIH- and NSF-supported papers were being cited with roughly the same frequencies by the prostaglandin patents. Private industry was probably the source for most of the articles in which no financial support was acknowledged (about half the cases).

In gas lasers, the federal government was the source of financial support in about 25 percent of the cases and industry in about 70 percent. The DoD was the largest agency source, at 18 percent.

This study shows that, at least in two technology fields, technological developments as revealed in patents draw heavily on scientific advances and that the federal government is the source of financial support for a significant proportion of the scientific advances on which technology draws. The study also shows that fields of technology vary in the extent to which they depend on or draw from science. Another relevant finding was that many of the cited references were not directly related to the immediate research field. For example, about 80 percent of references in prostaglandin patents were to organic chemistry and biochemistry papers not specifically related to prostaglandin research. The authors concluded "that the process of reduction to practice in industry continues to require recent science, and the support of such science is a necessary prerequisite for the continuing emergence of new technology."

CHI subsequently performed a validation study comparing patent citation and peer rankings of "science dependence" for 24 technologies.[5] The two rankings were found to have a high rate of agreement. Those technologies ranked by experts as highly science-dependent had a much higher average number of citations to journals per patent than did the technologies ranked by experts as having low science dependence. Testing for correlation in rank-ordering with Kendall's tau, they found that the number of references per patent to journals correlated highly with the expert judgments.

This type of research could be expanded to additional technologies. If Congress is interested in whether U.S. science is contributing to fields of technology that are particularly important to national welfare, defense, or international competitiveness, the method has potential for assessing the degree to which federally funded research is contributing to these technologies. Designed differently, a study could use the patent citation methodology to review the areas of science that are drawn upon in current patented technology developments.

The question these studies do not answer is whether science is contributing as much as it could or should to technology and how to change federal support for research to increase that contribution, if an increase is deemed desirable. While it might be tempting to try to reallocate funding in favor of areas of science that contribute to technological developments in important areas, it must be recalled that many of the citations were to basic research areas not directly relevant to the area of research. One area of research does not proceed in isolation from others, and advances in one may be delayed by lack of progress in others. We currently have an inadequate understanding of the nature of research and the interconnections among fields of research and the links of science to technology to guide policies that fund certain areas of science in the hope of near-term practical applications. Patent data can add somewhat to our understanding, but they alone are insufficient.

B. Studies of Patents Resulting from NSF Research Grants. The National Science Foundation has supported two studies attempting to determine the extent to which NSF grants led to patented technology and to estimate the economic value of those patents.

The first study was of the NSF chemistry program.[6] The names of NSF principal investigators (PIs) were matched with the PTO's computer list of inventor names on U.S. chemistry patents. The patents were then examined by a technical expert and compared to the technical details in the grant documentation. Economic value was estimated based on licensing data. A similar study was done for chemistry grants made by Research Corporation, a private foundation. In both programs, about one PI in 100 produced patents related to the grant, supporting the view that academics are not interested primarily in patenting. From a sample of 3,766 PI's, 95 related patents were issued to 39 PI's.

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[5] Mark P. Carpenter and Francis Narin, "Validation Study: Patent Citations as Indicators of Science and Foreign Dependence," World Patent Information, v. 5, no. 3, (1983): 180-185.

[6] Robert S. Cutler, "A Study of Patents Resulting From NSF Chemistry Program," World Patent Information, v. 6, no. 4 (1984): 165-169.

For each related patent, the study attempted to estimate the economic value by obtaining data on patent licensing from the assignee. Only 16 of the 95 related NSF patents (16 percent) were licensed or assigned to an industrial company. Another 36 were considered potentially licensable.

The study estimated the aggregate long-term sales of products from the related patents to be $20-30 million. That estimate must be regarded with considerable reservation for a number of reasons. Because licensing data are regarded as proprietary, not all parties were willing to share that information. Also, because the time lags in obtaining and licensing patents are significant, it was too early in the process to measure the full commercialization of many of the patents. Estimation of potential license income is especially shaky because of the difficulty of forecasting the market for the invention.

The fact that the number of related patents per PI were of comparable magnitude across the NSF and Research Corporation chemistry programs suggests that the low numbers are inherent in the nature of the research and not to program weaknesses. As noted in the previous section, much basic research does not result in patentable inventions. There are also other channels through which university basic research results are transferred into industrial technology. For example, when university research is financially supported by industry, research findings may be transferred into the company's research program without a patent.

Research projects may also contribute to subsequent technology developments, beyond the immediately resulting patent. One of the NSF PI's noted that a patent was issued as a result of the project and was licensed by the university to an industrial company which is still using the patented process. Subsequent to and as a result of the publication of the work done under the original grant, he and other investigators continued in the general research area under different sponsorship. Many important improvements have been made, and are now reportedly in commercial use.

The second NSF study, of the engineering research project grants funded 1968-1977, followed a similar methodology, but with some refinements such as increased effort to contact the inventors and assignees, and independent assessments of the economic value of relevant patents.[7]

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[7] SRI International, NSF Engineering Program Patent Study: Final Report, September 1985, prepared for the National Science Foundation.

Twenty percent of the PI's on NSF engineering research project grants produced patents related to their NSF grants, 7 were licensed and had produced royalties of $680,000 at the time of the study. A single patent earned almost all of those royalties--$620,000. This illustrates an important point about the economic value of patents. Many patents, regardless of origin, are of relatively little economic value. Occasionally, however, a patent is really important. The ferrite core, invented at MIT, for example, greatly increased the capacity and reliability of computer memories. In such cases the patent may be of great economic value and the economic return from that one patent will swamp the figures. These highly visible "big winners" may mislead universities and policymakers into unrealistically high expectations for the economic returns possible from patents. It also suggests that researchers should apply a different kind of statistics to patent studies, statistics more appropriate to predicting high-impact, low-frequency events, such as earthquakes.

Comparing the two NSF studies reveals that engineering research PI's were 20 times as likely as chemical research PI's to produce patents. (One percent of PI's in chemistry and 20 percent of PI's in engineering research produced related patents.) The rate at which resulting patents are licensed in the two programs is comparable: 17 percent for chemistry and 14 percent for engineering. Comparisons among scientific fields are interesting for what they may tell us about the nature of research in these fields and its connections to practical applications. These numbers taken alone do not justify a conclusion that engineering research is more productive than chemistry research and therefore deserves more funding, however. Chemistry and engineering science are not interchangeable in terms of the kinds of knowledge they produce.

Estimating the economic value of university patents is particularly problematic because universities are constrained in negotiating with potential licensees for a number of reasons. Universities lack the push that comes from a corporation's incentive to maximize the economic value received from technology it develops. The university is also less likely to be in the position of a manufacturing company, whose bargaining power is enhanced by its portfolio of patents and know-how related to the technology under discussion. The firm that licenses its patents has alternative means for realizing the economic value of the licensed technology, for example, through manufacturing and marketing it itself or through investing in another firm to do so. The existence of alternatives makes the industrial company more aggressive in negotiating licenses. Finally, many universities depend on industrial companies for gifts, research funding, and student employment. Bargaining too aggressively with a company for licensing royalties may sour the relationship between the school and the company, perhaps hurting the school more than the license royalty helps it.

Table 3 shows licensing data for the most active universities. One thing that is interesting about the numbers is they are all roughly $2 million, despite differences in in school size. This suggests that the limitation on licensing income is an administrative or institutional one, rather than related to the level of S&T activity.

TABLE 3

ANNUAL LICENSING INCOME FROM SELECTED TOP UNIVERSITIES

University	FY83 Licensing Income
Massachusetts Inst. of Technology	$1.6 million
University of California	2.3
Stanford University	2.1

Data Source: SRI International working papers

Interviews with university patent administrators confirmed the low observed patent commercialization rate. Many of them stated that maximizing license revenues is not their primary objective, but rather technology transfer. Universities also tend to look on patents more as a means of attracting further research support from industry. For example, MIT gets $27 million per year in industrial sponsorship, compared to about $2 million per year in patent royalties. Patents may create economic value through such relationships without licensing. In one industrially sponsored lab at a university, the industrial members pay an annual fee and are given royalty-free licenses to use any resulting inventions.

The method used to evaluate NSF research grants appears appropriate for tracing the movement of advances in scientific knowledge into applications in industrial technology. Even then it should be recognized that patents do not represent all the impacts that research may have on technology.

The method should probably not be used to compare different disciplines for purposes of evaluating the relative contributions to technology, for the same reason that patents are not used to compare the R&D outputs of firms in different industries. There are likely to be different propensities to patent inventions across disciplines and perhaps differences in patentability. However, it seems appropriate to make comparisons over time within a discipline or a program. Such studies could be used to evaluate the success of policies or programs intended to increase the transfer of science and technology from universities to industry, or from government funded R&D to the private sector. The method is potentially applicable, for example, to assessment of NSF Engineering

Research Centers, the University-Small Business Patent Amendments of 1980 (P.L. 96-517), and to the technology transfer functions of the federal laboratories.

As indicated above, significant problems exist in the use of royalties and licensing fees to estimate the economic value of research. They should probably not be used for that purpose. The whole area of measuring the economic value of patents is one that deserves further research.

As a closing note I would like to mention that much of the patent data used in Science Indicators and other S&T policy analyses in the past was provided by a small office in the Patent and Trademark Office known as the Office of Technology Forecast and Assessment or OTAF. That office was terminated last year as a budget measure, an action that seems particularly short-sighted in view of the growing need to analyze and evaluate our S&T capability and the shortage of objective indicators or S&T output. I believe that OTAF should be reestablished so that it can continue to provide patent data and analysis to government officials, policymakers such as yourselves, and the public. I would appreciate your support in seeking the reestablishment of this function. Thank you.

APPENDIX

United States Patent [19]

Anderegg et al.

[11] **Patent Number: 4,533,787**

[45] **Date of Patent: Aug. 6, 1985**

[54] **PRINTED CIRCUIT ASSEMBLY**

[75] Inventors: **Fredy Anderegg**, Lohn; **Josef Furrer**, Luterbach; **Heinz Däppen**, Flumenthal, all of Switzerland

[73] Assignee: **Autophon AG.**, Soleure, Switzerland

[21] Appl. No.: **440,999**

[22] Filed: **Nov. 12, 1982**

[30] **Foreign Application Priority Data**

Nov. 26, 1981 [CH] Switzerland 7559/81

[51] **Int. Cl.**^{3} **H05K 1/03**

[52] **U.S. Cl.** .... **174/68.5**; 174/117 A; 361/398; 361/412

[58] **Field of Search** .... 174/68.5, 117 A, 117 FF, 174/117 F; 361/412, 398

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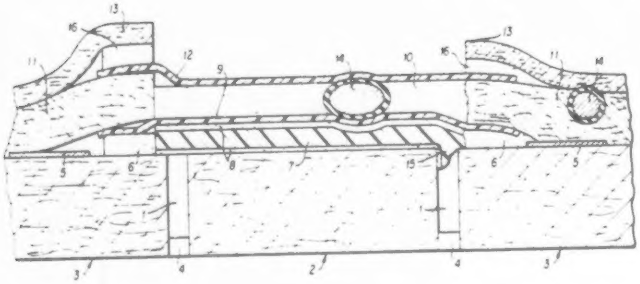
Primary Examiner—R. R. Kucia
Attorney, Agent, or Firm—Oblon, Fisher, Spivak, McClelland & Maier

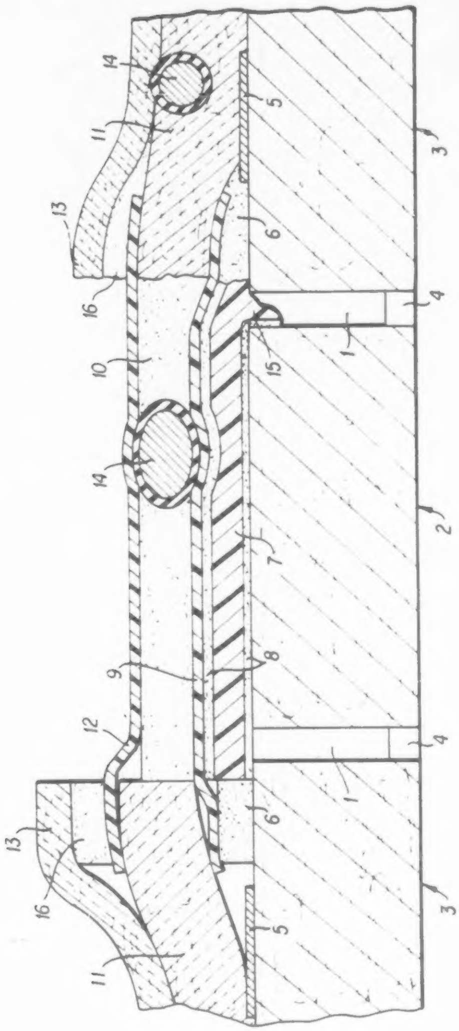
[57] **ABSTRACT**

Certain regions of a printed circuit assembly having mechanically laid wires are formed of two parallel sheets of polyimide film, the circuit assembly being flexible in those regions. The edges of these sheets of film are secured in the region of the edges of the rigid parts of the assembly by means of thermally activated adhesives. In the flexible regions, the wires are embedded in a thermally activated adhesive layer held between the two sheets of polyimide film. This adhesive layer, unlike adhesive layers on the rigid parts, contains no fabric.

2 Claims, 1 Drawing Figure



U.S. Patent Aug. 6, 1985 **4,533,787**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PRINTED CIRCUIT ASSEMBLY

This invention relates to electric circuits and wiring, and more particularly to a printed circuit assembly of the type having rigid and flexible parts, in which each two rigid parts are connected by a flexible part, and in which the rigid parts each include a lower first layer and an upper second layer, each containing a glass-fiber fabric and a thermally activatable adhesive, applied to a rigid board, mechanically laid wires being embedded in the first layer.

The prior art includes printed circuits made of rigid material to which at least two layers of a thermally activated adhesive are applied, with mechanically-laid insulated wiring being embedded in one of the layers and covered with the other. Such printed circuits are sold by Kollmorgen Inc., Glen Cove, N.Y., under the trademark "Multiwire". They have the advantage over conventional printed circuits that wire crossovers and a greater packing density are possible and the one-time preparations for a production run are fewer.

As in the case of conventional printed circuits, the walls of the holes must also be copper-plated in the case of wired printed circuits so that the leads inserted in these holes for connecting the components to be mounted can be properly soldered and also so that various levels of conductors can be connected via the holes. However, it is more difficult to apply such copper-plating in the case of wired printed circuits than in conventional ones. The latter are provided with holes before the conductor strips are applied, then immersed in a bath from which a very thin deposit of copper is precipitated by purely chemical means. The deposit in the holes is thereafter galvanically reinforced.

In the case of printed circuits having mechanically applied wires, however, such reinforcement by electroplating cannot be utilized. There the copper coating on the walls of the holes is produced by including in the material of the board and of the adhesive layers applied thereto a catalyst which, in cooperation with a suitable bath, makes it possible to deposit a sufficient thickness of copper by purely chemical means, without the use of electricity. The copper-plating is restricted to the holes by appropriate masking of the board.

Flexible "printed circuits" have been proposed which can be combined with rigid circuit boards in such a way that printed circuit assemblies having rigid parts with flexible parts between them can be produced without the use of soldered joints or plugs. Such printed circuits are taught, for example, in German Disclosed application (DOS) No. 26 57 212. In the case of the circuits described there, etched conductor patterns run uninterruptedly over rigid and flexible parts. They are manufactured starting from a known type of flexible "printed circuit" in which conductor patterns are produced by etching on one or both sides of a preferably polyimide film and covered by other films. Separate zones of such a flexible circuit are then cemented to or between rigid printed circuits, and the conductor strips of both types of circuits are interconnected through copper-plated holes.

It would seem logical to use the prior art technology also for printed circuits in which mechanically laid wires are embedded. However, the obstacles to doing so are, for one thing, that the polyimide films entering into consideration for the flexible parts do not contain any catalyst and, for another thing, that the material

used until now for embedding the wires is not flexible enough. This material must meet special requirements inasmuch as the wires are cemented to it, embedded in it, and are furthermore supposed to be secured against lateral displacement, and inasmuch as its surface should make possible good adhesion of the wires when laid as well as the formation of a strongly adhering copper coating; it contains a glass-fiber fabric which is primarily responsible for the aforementioned drawback.

Hence polyimide film cannot be used in the rigid parts, where there must be holes with copper-plated walls, while the material in which the wires are embedded on the rigid base plates is not usable in the flexible part.

The present invention is based upon the realization that no holes need be bored in the flexible part and that the requirements to be met by the material for the flexible parts are much lower than for the corresponding material on the rigid parts. Hence it follows that other materials can be used for the flexible parts than for the rigid parts insofar as the use of these materials is limited to the flexible parts. Thus, in the flexible regions, polyimide film can be used as the substrate, and for embedding the wires materials can be used, which besides flexibility, need only meet the requirement that the wires adhere to it somewhat when being laid.

It is an object of this invention to provide a printed circuit assembly having wiring mechanically laid on rigid and flexible parts made of different materials, the wiring running over these different parts being nonetheless continuous, as well as a method of manufacturing such an assembly.

To this end, in the printed circuit assembly according to the present invention, of the type initially mentioned, the flexible parts each comprise a non-adhesive, flexible upper film and lower film running solely between the rigid parts and attached in edge zones of the rigid parts, and there are wires which run uninterruptedly from one of the rigid parts over a flexible part to the adjacent rigid part and, in so doing, are embedded in the flexible part in a third layer which is inserted between the two non-adhesive films, connects the two films, and is made of a fiber-free, thermally activatable adhesive.

In one embodiment of the invention, further adhesive layers are applied in the mentioned edge zones for attaching the edges of the non-adhesive films.

A preferred embodiment of the invention will now be described in detail with reference to the accompanying drawing, which is a cross-section through a three-part printed circuit assembly in one of the last phases of manufacture.

It should be noted in connection with the drawing that the thicknesses of the layers are shown on a scale approximately 20 times greater than that of the lengths, except as concerns the wires. The wires running perpendicular to the plane of the drawing are round; those which run parallel to the plane of the drawing and can therefore not be shown correctly are depicted as being cut at an angle. Hence the oval shape of the latter does not indicate any deformation of these wires, as might wrongly be supposed. On the left-hand side of the drawing, the layers are shown as they lie one upon the other in their uncompressed state, whereas the right-hand side shows the situation after compression and bonding.

The drawing shows a rigid base plate or board of glass-fiber-reinforced epoxy material which is divided by slots 1 into an inner region 2 and outer regions 3. Except for crosspieces 4, slots 1 are continuous. Board parts 2 and 3 are temporarily held together by means of crosspieces 4 but can easily be broken apart. The two outer parts 3 of the board subsist as rigid printed circuits, whereas the middle part 2 is subsequently removed.

Parts 3 exhibit a conductor pattern 5 applied conventionally by etching; this conductor structure serves the primary purpose of supplying power and grounding and has nothing to do with the operations and arrangements to be described hereinafter.

Regions 3 of the board, forming the rigid parts, are fundamentally each provided with a first layer 11 of material suitable for receiving wire 14 embedded therein, and with a second layer 13 of covering material. This structure corresponds to a normal rigid printed circuit with inlaid wires. Both layers 11 and 13 contain a glass-fiber fabric and a thermally activated epoxy-resin adhesive; layer 13 is of the material of this type known in the trade as "prepreg". The glass-fiber fabric in the first layer 11 prevents lateral displacement of the wires, and its composition makes possible faultless copper-plating of holes (not shown) and connection of this layer of copper to the wires intersected by the holes.

Inserted between the base plate and the first layer 11, and between the first and second layers 11, 13, in the area of an edge zone, are sheets 9 and 12, respectively, of the non-adhesive polyimide film sold under the trademark "Kapton" by E. I. du Pont de Nemours & Co., Wilmington, Del. These films are attached within the edge zones to board part 3 and to layer 13 by adhesive layers 6 and 16, respectively. Layers 6 and 16 are likewise of thermally activated material, preferably of the acrylate material sold under the trademark "Pyralux LF 0100" by E. I. du Pont de Nemours & Co.

By bonding polyimide films 9 and 12 with the aid of strips 6 and 16, and owing to the glass-fiber fabric contained in layers 11 and 13, a high-strength connection between board parts 3 and films 9 and 12 is obtained.

Polyimide films 9 and 12 are bonded together by a layer 10 of the fiber-free, thermally activated adhesive sold under the trademark "Scotch-Weld 588" by Minnesota Mining and Manufacturing Company, St. Paul, Minn. This adhesive, a mixture of synthetic elastomer and thermosetting resin, is generally used for cementing brake linings and clutch facings or for gluing signs to a supporting structure.

Embedded in layers 11 and 10 are mechanically laid wires 14 which run uninterruptedly from one of the rigid parts 3 over the flexible part 2 consisting of films 9 and 12 with adhesive layer 10 between them to the other rigid part 3 and are thus undetachably secured to all three parts of the printed circuit board.

A film 7, 8, needed only during manufacture of the printed circuit assembly, will be explained below in that context.

A printed circuit assembly as shown in the drawing is manufactured as follows: first slots 1 are milled into the base plate so that only crosspieces 4 remain as connections between parts 2 and 3. The conductor pattern 5 is then produced in a conventional manner.

A composite film consisting of a thermoplastic layer 7 between two sheets of non-adhesive separator film 8 is laid on middle part 2. This composite film, sold under the trademark "Contiflex Conformal Coating" by Contiflex AG, Küsnacht-Zürich, Switzerland, is normally used in compressing uneven layered structures. The dimensions of composite film 7, 8 are such that it completely covers over slots 1 and extends a bit farther.

Since film 8 may stick to the "Kapton" film 9 during the operations to be described below, it is advisable to lay another separator film, viz., a sheet (not shown) of the polyvinylidene fluoride film sold under the trademark "Tedlar" by E. I. du Pont de Nemours & Co., upon composite film 7, 8.

The lower polyimide film 9 is then laid on the middle part 2 in such a way that it extends about 5 mm. beyond the edges of film 7, 8. Between this projecting portion of film 9 and board part 3, the adhesive strip 6 of the aforementioned acrylate material "Pyralux LF 0100" is inserted. This fiber-free, thermally activated adhesive is customarily used in conventional flexible printed circuits to bond the "Kapton" films, also used there, to a rigid substrate, or to join two "Kapton" films.

Strip 6 is provisionally attached to board part 3 or to film 9 by spot-heating, e.g., with a soldering iron, before film 9 is put in place.

Sheet 10 of the aforementioned fiber-free, thermally activated adhesive film "Scotch-Weld 588", corresponding in size and positioning to composite film 7, 8, is laid upon polyimide film 9. The adhesive film 11 intended to receive the wires in wired printed circuits, containing glass-fiber fabric and likewise thermally activated, is now laid upon each of the parts 3 in such a way that the edges of films 10 and 11 abut one another.

In order to avoid mutual displacement, the stacked films are temporarily attached until the next processing step, partly by spot-heating, partly by tape which is adhesive on both sides.

The stack thus far assembled is then compressed, using suitable resilient, adhesive-repellant underlying supports, at a temperature at which the thermally activated layers just begin to flow. The discontinuity caused by the differing thicknesses of films 10 and 11 is evened out somewhat at this time.

Next, insulated wires 14 are mechanically laid in layers 11 and 10 in a manner known per se but having no direct connection with the present invention. This wire-laying operation is carried out by moving the base plate or board under a presser-foot, the wires being superficially embedded in layers 11 and 10 by vibration of the foot at supersonic frequency as a result of the heat then produced. The starting and end points and the run between them are computer-controlled. In the regions of parts 3, the wires run parallel and perpendicular to the plane of the drawing, while in the region of middle part 2, the wires run only parallel to that plane.

After completion of the wiring, non-adhering polyimide film 12 is laid over middle part 2 with its edges extending beyond underlying layer 10 in the same way as in the case of film 9. Either before or after film 12 is put in place, these projecting edges are joined to thermally activated strips 16 of the aforementioned adhesive "Pyralux LF 0100", corresponding to strips 6.

Finally, the two fiber-glass-reinforced, thermally activated "prepreg" films 13 are laid over the areas of circuit board parts 3, the size and positioning of these films corresponding to those of films 11.

After all the films have been applied, the entire stack is again compressed between suitable deformable, adhesive-repellant supports under the influence of heat, at which time the various activatable layers pass into their final state, and the wires become embedded in their underlayers to the extent shown in the drawing. The wires lying in layers 11 cannot be displaced during the compression operation owing to the effect of the fabric inlay; this is important if the course of the wires is to match precisely the pattern of holes to be made subsequently. The wires disposed in adhesive layer **10**, on the other hand, are not secured against displacement during compression; this is no disadvantage, however, since no holes are bored in that region. The danger that the wires in the flexible part which are situated near slots **1** might be pushed downward and damaged during compression is precluded by the properties of composite film **7**, **8**; for when this film is heated and compressed, thermoplastic material emerges as shown at the location indicated by reference numeral **15**, partially fills slot **1**, and thereby counteracts the sagging of layers **9**, **10**, and **12**. For appreciating this situation, the different scales used in the drawing for two directions, as explained earlier, must be taken into account.

In a manner not shown in the drawing, the printed circuit assembly as described is then covered with an additional film, and the prescribed holes which intersect wires **14** and copper conductors **5** are bored in the regions of parts **3**. As already mentioned, the material of which board parts **3** and layers **11** and **13** are made contains a catalyst, while the aforementioned additional film does not. If the assembly is now immersed in a bath which copper-plates all material containing the catalyst, copper will be deposited only on the walls of the holes. After the copper-plating operation, the lastapplied film is removed.

The printed circuit assembly is thereafter equipped with discrete components by inserting the leads of these elements into the copper-plated holes. The connections are then dip-soldered in a manner known per se.

Next, crosspieces **4** are broken, whereupon the middle part **2** of the board, together with composite film **7**, **8** and possibly the non-illustrated separator film, can be removed. Thus, the printed circuit assembly consists of two rigid boards connected by a flexible portion.

The invention is naturally not confined to the embodiment described above. In particular, there are presumably other thermally activated adhesive films besides "ScotchWeld 588" and "Pyralux LF 0100" which possess the appropriate properties for carrying out the invention and which might be used instead of the materials named. "Contiflex Conformal Coating" might likewise be replaced by another material having similar properties.

Although the embodiment illustrated and described comprises a three-part printed circuit assembly having a flexible part between two rigid parts, it is quite possible to connect more than two rigid parts by such interposed flexible parts. Any desired mutual arrangement of the rigid parts, and hence of the shapes and positions of the flexible parts, may be chosen.

Insofar as it is necessary to incorporate such an extensive amount of wiring that a single layer does not suffice, further wired layers (**11**) can be disposed one above the other in such regions in separate operations.

What is claimed is:

1. In a circuit board assembly comprising a plurality of rigid parts and at least one flexible part connecting each two said rigid parts, said flexible part comprising an upper and a lower non adhesive flexible film, the board further comprising wires, at least a part of these wires running uniterruptedly from one of said rigid parts over said flexible part to an adjacent one of said rigid parts, said wires in all parts being embedded in a layer of thermally activatable adhesive, said adhesive is, in the flexible parts, disposed between the non adhesive films, and, in the rigid parts, disposed on a rigid base plate and, including the wires, covered by a cover layer, the improvement that the non adhesive films extending solely between said two rigid parts and being secured thereto at the edges thereof and that two kinds of adhesive layers are disposed, the layer on said rigid parts containing a glass-fiber fabric and the adhesive in said flexible part is fiber-free.

2. The printed circuit assembly of claim 1, wherein an edge of each said first layer overlaps a respective edge of the one of said two sheets of film closer to said base plate, the edges of the other of said two sheets of film overlap respective edges of each said first layer, and an edge of each said second layer overlaps a respective edge of said other of two said sheets of film, each of said rigid parts further including a first thermally activated adhesive strip inserted between an edge of said base plate and said respective edge of said one of said two sheets of film, and a second thermally activated adhesive strip inserted between said edge of said second layer and said respective edge of said other of two said sheets of film.

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PATENTS AND INNOVATION FORECASTING

by

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PATENTS AND INNOVATION FORECASTING

ABSTRACT

If one accepts the definition that "a forecast is a statement about the future, based on data about the past," patent data would appear to lend themselves well to innovation forecasting. Patents are one of the few available quantitative, objective indicators of innovative activy. They cover a broad range of technologies and most countries; long time series exist; and they provide detailed information on type of technology, inventors, owners, location, prior art, and application and issue date. In addition, advances in information technology and the U.S. Patent and Trademark Office program of disseminating patent information and encouraging its use for various analytical purposes have made patent data much more accessible and useful.

This paper reviews selected literature items that appeared from their titles to deal with patents and forecasting. Some of these articles were found in fact to make or deal with forecasts. The others used patent data to describe past and current trends in technology, with an informal allusion to the possibility of forecasting on the basis of these trends. The paper concludes that use of patents for technology forecasting is not well developed.

Patents are being used extensively, however, as indicators of innovative activity. The paper goes on to describe these uses, relying heavily on a recent review by Keith Pavitt of the University of Sussex. Analytical approaches include monitoring of trends and international comparisons typified by Science Indicators; bibliometric techniques for studying links between science and technology, between firms, and between nations; and economic studies of the determinants and consequences of technological change. Patent statistics have been used to make comparisons across technologies, firms, industries, countries, and over time.

The use of patent data for these purposes has not been without its criticisms. The next section of the paper reviews the criticisms and the progress that has been made in understanding the shortcomings of patent data and ways of dealing with those shortcomings.

The paper concludes that most of the intellectual energy in this field to date has been channeled into examination of the adequacy of patent data for measuring and describing technological change. Much of the work that has been done has been "experimental" in the sense of exploring the feasibility of using patents for certain purposes, the problems associated with use, and ways of overcoming these problems. The validity of patents as technology indicators is fundamental and logically prior to their use in technology forecasting. The paper argues that patent data can be a satisfactory indicator of technological innovation; in some cases they are as good as, or better than, data on research and development expenditures. One might now expect more attempts to use patent data for innovation forecasting. The last section describes some types of forecasts that may be possible.

I. INTRODUCTION: PATENTS AS ANALYTICAL DATA

Patents represent a wealth of technological data. A copy of a U.S. patent is included in the Appendix. Each patent contains the following information:

1. By law the text of the specification of the patent (the patent "disclosure") must provide sufficient information to enable one "skilled in the art" to use the invention. The claims of the patent set out the legal definition of the invention.

2. The front page of the patent contains what is called "bibliographic" information--that is, the patent number, the application number, the patent title, the name and address of the inventor, the name of the assignee at time of patent issue, if any; application and issue date, foreign priorities, if any; and U.S.Patent Classification class and subclass.

3. The front page of the patent also contains references to previous patents or technical literature cited by the patent examiner as the nearest prior art to the invention in the patent. These references are representative of the scientific or technical base upon which the patented invention is building.

In the United States alone, over four and one-half million patents have been issued since the first patent was issued in 1790. U.S. patents are organized by the U.S. Patent Classification (USPC) system into about 400 classes and about 115,000 subclasses of technology based on the structure and function of the patented invention. In terms of the length of time period covered, detail, and range of technologies covered, patent data are not equaled.

Patents are one of the few available quantitative, objective indicators (i.e., indirect measures) of technological change. Other available data such as expenditures for research and development (R&D) and numbers of scientists and engineers, measure the resource inputs to science and technology, but not the output of those activities. Patents, which are an output of technological activity, ar thus particularly valuable indicators.

Until the early 1970s, patent data were not very accessible. Access required manual searches of the huge patent file or through the Official Gazzette, a PTO weekly publication of patent bibliographic information. Although the economist Jacob Schmookler did some pioneering work in the 1960's on the relation of technological change to investment using patent data, this sort of work was limited.[1]

With the computerization of patent records, substantial progress has been made in improving the availability and utility of patent data. The U.S. Patent and Trademark Office (PTO) led in the move to computerize patent records. The Technology Assessment and Forecast program was established in the PTO in 1971 to promote the dissemination and use of patent information. It was administered by the Office of Technology Assessment and Forecast (OTAF).

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[1] J. Schmookler, Invention and Economic Growth (Cambridge, MA: Harvard University Press, 1966).

The elements of the OTAF data base were extracted from the information on the front pages of the patents. Coverage began with patents issued in 1963. In 1985 the OTAF data base contained the following elements:

Technological category (USPC classification)
Ownership category (i.e., corporate, government, or individual)
Inventor state or country
Application date
Patent title
Patent number
Patent issue date
Assignee (owner) name
Inventor name and address
Field of search (USPC subclasses searched by the examiner)
References cited by the examiner

In addition, OTAF developed a concordance between the USPC and the Standard Industrial Classification (SIC) system that enabled patents to be related, on the basis of their USPC subclass assignment, to product fields at the level of two- and three-digit SIC codes.

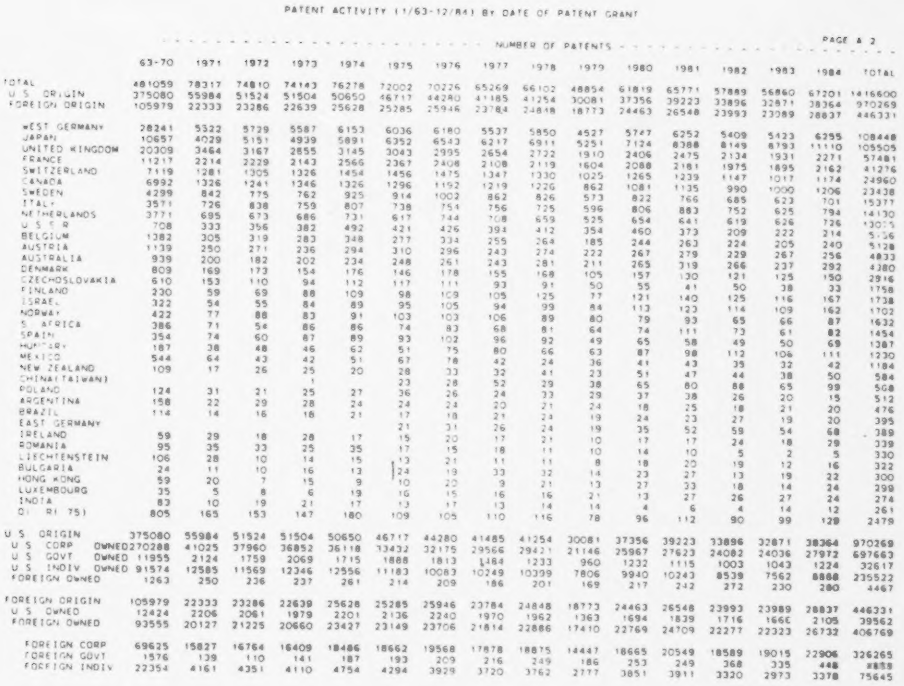
OTAF designed a series of standard report formats -- such as the Technology Profile and the Organizational Profile -- that provided patent count information broken down in useful ways. Using the Technology Profile format, OTAF could generate a report on any technology definable in terms of the USPC. Using the Organizational Profile, it could generate a report on the patenting activity of any organization to which U.S. patents had been assigned. One of the most widely cited OTAF reports was the "All Technologies" report, which showed total yearly U.S. patenting levels broken down by country of origin and type of ownership -- i.e., corporate, individual, or government. One page from the 1984 All Technologies report is shown in Figure 1.

Graphing the data in the All Technologies report shows some of the major trends in U.S. patenting. Figure 2 shows, for example, the decline in total U.S. patenting from 1971 to 1983. Most of this decline is due to declining U.S.-origin patenting, while foreign-origin patenting is increasing. The graph also illustrates some of the problems in using patent issue data, as opposed to patent application data. For example, the large dip in patents issued in 1979 was due to a shortage of PTO funds for printing patents that year.

Some of these problems are solved by using patent application data, because they are not affected by patent office processing. Figure 3, which shows U.S. patent filing (i.e., application) trends, shows that the trends in applications are much less erratic than patent issues. Patent application information is not available at the USPC subclass level, because individual patent applications are regarded as proprietary information until a patent is issued. OTAF developed an

Figure 1

SAMPLE PAGE FROM 1984 ALL TECHNOLOGIES REPORT



PATENT ACTIVITY (1/63-12/84) BY DATE OF PATENT GRANT

PAGE A 2

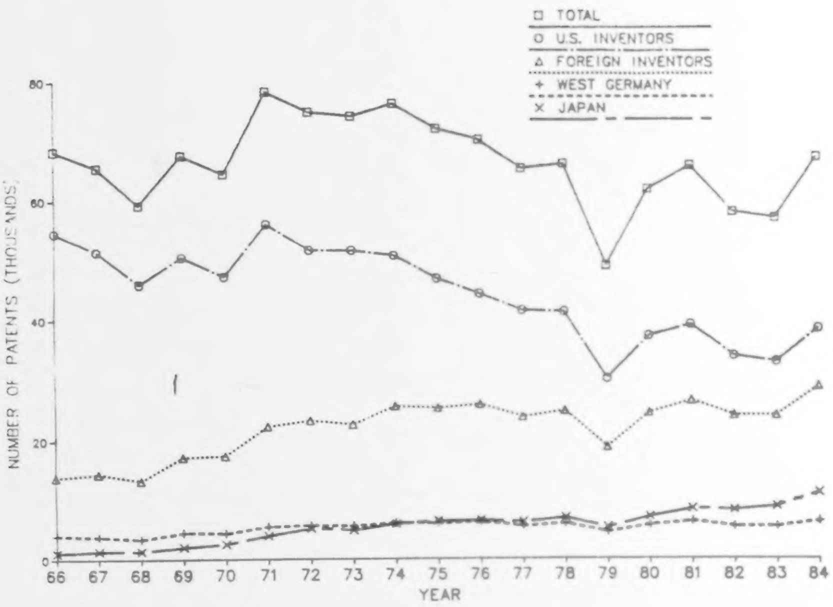
NUMBER OF PATENTS

	63-70	1971	1972	1973	1974	1975	1976	1977	1978	1979	1980	1981	1982	1983	1984	TOTAL
TOTAL	481059	78317	74810	74143	76278	72002	70226	65269	66102	48854	61819	65771	57889	56860	67201	1416600
U S ORIGIN	375080	55984	51524	51504	50650	46717	44280	41185	41254	30081	37356	39223	33896	32871	38364	970269
FOREIGN ORIGIN	105979	22333	23286	22639	25628	25285	25946	23784	24848	18773	24463	26548	23993	23089	28837	446331
WEST GERMANY	28241	5322	5729	5587	6153	6036	6180	5537	5850	4527	5747	6252	5409	5423	6255	108448
JAPAN	10657	4029	5151	4939	5891	6352	6543	6217	6911	5251	7124	8388	8149	8793	11110	105505
UNITED KINGDOM	20309	3464	3167	2855	3145	3043	2995	2654	2722	1910	2406	2475	2134	1931	2271	57481
FRANCE	11217	2214	2229	2143	2566	2367	2408	2108	2119	1604	2088	2181	1975	1895	2162	41276
SWITZERLAND	7119	1281	1305	1326	1454	1456	1475	1347	1330	1025	1265	1239	1147	1017	1174	24960
CANADA	6992	1326	1241	1346	1326	1296	1192	1219	1226	862	1081	1135	990	1000	1206	23438
SWEDEN	4299	842	775	762	925	914	1002	862	826	573	822	766	685	623	701	15377
ITALY	3571	726	838	759	807	738	754	756	725	596	806	883	752	625	794	14130
NETHERLANDS	3771	695	673	686	731	617	744	708	659	525	654	641	619	626	726	13075
U S S R	708	333	356	382	492	421	426	394	412	354	460	373	209	222	214	[illegible]
BELGIUM	1382	305	319	283	348	277	334	255	264	185	244	263	224	205	240	5128
AUSTRIA	1139	250	271	236	294	310	296	243	274	222	267	279	229	267	256	4833
AUSTRALIA	939	200	182	202	234	248	261	243	281	211	265	319	266	237	292	4380
DENMARK	809	169	173	154	176	146	178	155	168	105	157	130	121	125	150	2916
CZECHOSLOVAKIA	610	153	110	94	112	117	111	93	91	50	55	41	50	38	33	1758
FINLAND	230	59	69	88	109	98	109	105	125	77	121	140	125	116	167	1738
ISRAEL	322	54	55	84	89	95	105	94	99	84	113	123	114	109	162	1702
NORWAY	422	77	88	83	91	103	103	106	89	80	79	93	65	66	87	1632
S AFRICA	386	71	54	86	86	74	83	68	81	64	74	111	73	61	82	1454
SPAIN	354	74	60	87	89	93	102	96	92	49	65	58	49	50	69	1387
HUNGARY	187	38	48	46	62	51	75	80	66	63	87	98	112	106	111	1230
MEXICO	544	64	43	42	51	67	78	42	24	36	41	43	35	32	42	1184
NEW ZEALAND	109	17	26	25	20	28	33	32	41	23	51	47	44	38	50	584
CHINA(TAIWAN)				1		23	28	52	29	38	65	80	88	65	99	568
POLAND	124	31	21	25	27	36	26	24	33	29	37	38	26	20	15	512
ARGENTINA	158	22	29	28	24	24	24	20	21	24	18	25	18	21	20	476
BRAZIL	114	14	16	18	21	17	18	21	24	19	24	23	27	19	20	395
EAST GERMANY						21	31	26	24	19	35	52	59	54	68	389
IRELAND	59	29	18	28	17	15	20	17	21	10	17	17	24	18	29	339
ROMANIA	95	35	33	25	35	17	15	18	11	10	14	10	5	2	5	330
LIECHTENSTEIN	106	28	10	14	15	13	21	11	11	8	18	20	19	12	16	322
BULGARIA	24	11	10	16	13	24	19	33	32	14	23	27	13	19	22	300
HONG KONG	59	20	7	15	9	10	20	9	21	13	27	33	18	14	24	299
LUXEMBOURG	35	5	8	6	19	16	15	16	16	21	13	27	26	27	24	274
INDIA	83	10	19	21	17	13	17	13	14	14	4	6	4	14	12	261
OTHER (75)	805	165	153	147	180	109	105	110	116	78	96	112	90	99	129	2479
U S ORIGIN	375080	55984	51524	51504	50650	46717	44280	41485	41254	30081	37356	39223	33896	32871	38364	970269
U S CORP OWNED	270288	41025	37960	36852	36118	33432	32175	29566	29421	21146	25967	27623	24082	24036	27972	697663
U S GOVT OWNED	11955	2124	1759	2069	1715	1888	1813	1484	1233	960	1232	1115	1003	1043	1224	32617
U S INDIV OWNED	91574	12585	11569	12346	12556	11183	10083	10249	10399	7806	9940	10243	8539	7562	8888	235522
FOREIGN OWNED	1263	250	236	237	261	214	209	186	201	169	217	242	272	230	280	4467
FOREIGN ORIGIN	105979	22333	23286	22639	25628	25285	25946	23784	24848	18773	24463	26548	23993	23989	28837	446331
U S OWNED	12424	2206	2061	1979	2201	2136	2240	1970	1962	1363	1694	1839	1716	1666	2105	39562
FOREIGN OWNED	93555	20127	21225	20660	23427	23149	23706	21814	22886	17410	22769	24709	22277	22323	26732	406769
FOREIGN CORP	69625	15827	16764	16409	18486	18662	19568	17878	18875	14447	18665	20549	18589	19015	22906	326265
FOREIGN GOVT	1576	139	110	141	187	193	209	216	249	186	253	249	368	335	448	[illegible]
FOREIGN INDIV	22354	4161	4351	4110	4754	4294	3929	3720	3762	2777	3851	3911	3320	2973	3378	75645

Source: Office of Technology Assessment and Forecast, Patent and Trademark Office, U.S. Department of Commerce

Figure 2

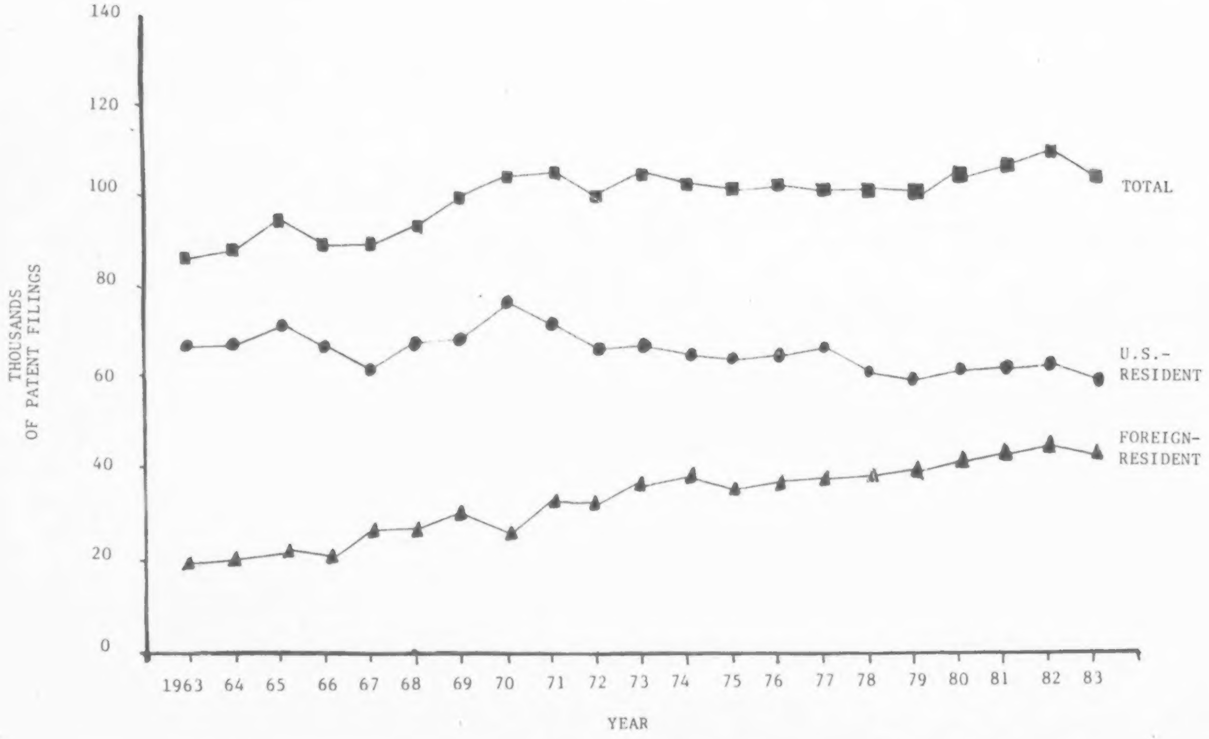
NUMBER OF U.S. PATENTS ISSUED 1966-1984
BY NATION OF ORIGIN



Source: Office of Technology Assessment and Forecast, Patent and
Trademark Office, U.S. Department of Commerce

Figure 3

U.S. PATENT FILING TRENDS
1963-1983



Source: Office of Technology Assessment and Forecast, Patent and Trademark Office, U.S. Department of Commerce indicator called "patented applications," which is essentially patent issue data redistributed by application date. This indicator has the advantage of not being affected by patent office processing variations and yet can be broken down by technology classification.

In addition to the OTAF data base, other sources of patent data have developed. Selected sources are listed in Figure 4. Some are based on U.S. patents or other national patent systems, as in the case of the Canadian PATDAT data base or the Japan Patent Information Center (JAPATIC). Others are international patent data bases, such as that of the World Intellectual Property Organization (WIPO) and the International Patent Documentation Center (INPADOC). Some are run by public organizations, such as national governments, while others are proprietary, such as DERWENT LTD. Some of them are intended primarily for statistical purposes, such as the OTAF data base, while others are intended primarily for patent searching, such as PATSEARCH. Some are the results of academic research projects and include other data in addition to patenting, such as the Harvard-National Bureau of Economic Research data base. There are also many online patent data bases.[2] Pergamon's PATSEARCH allows key word searching. Mead Data Central's LEXPAT offers full-text searching. For purposes of studying innovative activity, the OTAF data base is probably the most developed in terms of the length of time series, the amount of data available, and the flexibility of formatting.[3]

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[2] A list of 89 databases that cite patents and are available online to the public may be found in E Simmons and F. Rosenthal, "Patent Databases: A Survey," World Patent Information, v. 7, no. 1/2, 1985: 33-67.

[3] Unfortunately, the OTAF program was terminated in the spring of 1985 as a budget measure. It is unknown at this time whether its data services will be offered again. Further information concerning the availability of patent information from the Patent and Trademark Office can be obtained from the Administrator for Documentation, Patent and Trademark Office, Washington, D.C. 20231.

Figure 4

SELECTED SOURCES OF PATENT INFORMATION

PUBLIC	U.S. Patent and Trademark Office Contact: Jane Myers Documentation Organizations
	Canada -- PATDAT World Intellectual Property Organization (WIPO) International Patent Documentation Center (INPADOC) European Patent Office (EPO) Japanese Patent Information Center (JAPATIC)
PRIVATE	DERWENT, LTD. Computer Horizons, Inc. Battelle Pacific Northwest Laboratories
ACADEMIC	Harvard/National Bureau of Economic Research Contact: Prof. Zvi Griliches
	Yale University Contact: Prof. Richard Levin
	Swarthmore College Contact: Prof. F.M. Scherer
ONLINE	PATSEARCH (Pergamon) LEXPAT (Mead Data Central) CLAIMS (IFI?Plenum) World Patent Index (Derwent)

II. PATENTS AND INNOVATION FORECASTING: A REVIEW

One definition of a forecast is "a statement about the future, based on data about the past."[4] In today's world, in which rapidly changing technology creates great promise but also great uncertainty, it is increasingly important to be able to make forecasts about technological change. Given their improved accessibility and the existence of fairly long time series, patent data would seem to be a likely basis from which to make such forecasts. This possibility was strongly suggested by Richard S. Campbell, based on his study of automobile patenting. He wrote:

> Foreign automakers signaled the United States 10 years ago that their fuel-efficient automobiles would give American cars a run for their money. But American companies weren't listening. American car manufacturers might have been ahead of the game had they recognized the significance of a burst of patenting activity by German and Japanese automobile manufacturers. This signal in the early 1970s should have alerted U.S. firms that their overseas competitors were developing advanced fuel-control technology. But American car manufacturers did not know how to interpret the wealth of information found in the patents their competitors were filing at the U.S. Patent and Trademark Office.[5]

This section of the paper reviews works by Campbell and others that deal with the use of patent data for forecasting. The items reviewed include:

1. Richard S. Campbell, Patent Trends as a Technological Forecasting Tool, Workshop on Patent and Innovation Statistics, Organization for Economic Cooperation and Development, Paris, June 28-30, 1982; "Patenting the Future," The Futurist (December 1983): 62-67
2. Office of Technology Assessment and Forecast, Early Warning Report, U.S. Department of Commerce, Patent and Trademark Office, December 1973
3. Office of Technology Assessment and Forecast, "Forecasting Patent Activity by Use of Pending Patent Application Data," Seventh Technology Assessment and Forecast Report, U.S. Department of Commerce, Patent and Trademark Office, March 1977
4. Office of Technology Assessment and Forecast, Design Patents, U.S. Department of Commerce, Patent and Trademark Office, 1983
5. Joseph P. Martino, et al., Tests of Some Indices of Technological Change as Leading Indicators of Market Shift, (Dayton, OH: University of Dayton Research Institute, February 1980) prepared for the National Science Foundation under grant PRA78-20313

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[4] Joseph P. Martino, Technological Forecasting for Decision Making, 2d ed. (New York: North-Holland, 1983), p. 250.

[5] Richard S. Campbell, "Patenting the Future: A New Way to Forecast Changing Technology," The Futurist (December 1983): 62.

6. Joseph Martino, Chapter 9, "Forecasting Technological Breakthroughs," in Technological Forecasting for Decision Making, 2d ed. (New York: North-Holland, 1983)

7. Rudolf Hack and Libor Matejka, "Prognosis of the Development of Artificial Kidneys Based Upon Patent Literature," World Patent Information, v. 4, 1982: 23 - 32

8. Hanna Mlodzik, "Patent Literature -- A Tool for Forecasting in the Pharmaceutical Industry?" nd, np (an adapted reprint of a paper presented at the DERWENT Annual CPI Meeting, London, May 1979)

9. Konrad Faust, "Patentdaten als Frühindikatoren der Technologischen Position Konkurrierender Industrieländer," IFO-Schnelldienst, 1981

Each work is described briefly and then a summary assessment is offered.

1. Campbell, "Patents as a Technology Forecasting Tool" and "Patenting the Future"

Although Campbell claimed that U.S. automobile manufacturers should have been able to predict German and Japanese fuel-efficient technology from patenting by those countries in the United States 10 to 15 years previously, his own work did not involve any formal forecasting using patent data.

The prediction about automobile technology was a very informal and subjective one, based on Campbell's observation while doing a patent search at the PTO. He wrote elsewhere:

> I noticed that you could look down the miles of patents stacked in the patent office and tell where the action was by the color of the patents' edges. Old patents were yellow; new patents were white. It was clear from the names in the white patents in the fuel control technology -- Nissan, Mitsubishi, Fiat, BMW, Volvo, Mercedes -- that they were responding to something that U.S. firms were not.[6]

Campbell's work basically addressed the need that companies have for objective indicators of their technological position, performance, and prospects. He and his colleagues at Battelle Pacific Northwest Laboratories worked at a disaggregated level -- specific technical problems and their candidate solutions, defined in terms of the USPC. Data were manually extracted from the individual patents and run through special statistical analyses to calculate a variety of indicators. Battelle developed about 60 indicators, representative of ten technical dimensions. Three of these dimensions were activity, immediacy, and concentration. The activity dimension was measured by the number of new firms in the patent area, the number of active firms, the number of dropouts, the number of new and old inventors and those no longer active, as well as by the traditional number of patents in a given period. Immediacy was measured by the age of the closest prior art in the documents cited in patents. If the closest prior art was very recent, Battelle inferred that it was a rapidly growing technology. Concentration refers to the degree of concentration in the production of patents by firms and inventors. It is assumed that when a technology first develops concentration is high.[7] Campbell postulated that these three indicators could be used to identify the stage of development of a technology as emerging, growing, maturing, or in obsolescence. (See Figure 5.)

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[6] Campbell, Patent Trends as a Technological Forecasting Tool, np.

In another article, Campbell offered suggestions for interpreting patenting trends in terms of forecasting. For example, "if most of the patents cited are recent, we can expect very rapid technological growth in this area." And, "if a firm is producing a large number of defensive and auxiliary patents around a basic invention, it indicates a possible future thrust." However, the empirical basis for these interpretations was not revealed.

Campbell acknowledged certain limits to patent analysis. He pointed out that government- or military-dominated technologies are not generally good areas for analysis because there are not usually enough patents to analyze. This is also true in areas where the firms are concerned about antitrust action. For example, IBM publishes information in the open literature about patentable inventions to avoid antitrust problems, while preventing other firms from locking it out. Also, technologies with short track records are not good candidates for patent analysis.

2. OTAF Early Warning Report

The OTAF Early Warning Report used data on the yearly number of patents issued to forecast future numbers of patents issued. The report assumed that "if the number of U.S. patents in a given area is growing rapidly," then "technological advance in that area is being actively pursued."[8]

The report presented data on the number of patents issued for each year from 1963 - 1972 and calculated the average percentage growth for 1970 - 1973, that is, the number of patents added as a percentage of existing patents. It then projected an average percentage growth for the years 1973 - 1977. In the same way, it calculated an average percentage foreign-origin for the years 1970 - 1973 and projected an average percentage foreign-origin for the years 1973 - 1977. The projection was determined through computer extrapolation of the

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[7] Richard S. Campbell and A.L. Nieves, Technology Indicators Based on Patent Data: The Case of Catalytic Converters, prepared by Battelle Pacific Northwest Laboratories for the National Science Foundation, September 1979, p. 4.2.

[8] OTAF, Early Warning Report, p. A-1.

Figure 5

TECHNOLOGICAL STAGE OF DEVELOPMENT

Indicator → Stage ↓	Activity	Immediacy	Concentration
Emerging	Low, Increasing	Increasing	Low
Growing	High	High	Increasing
Maturing	Stable	Decreasing	Increasing
Obsolescence	Low and Decreasing	Low and Decreasing	High and Increasing

Source: Richard S. Campbell, Patent Trends as a Technological Forecasting Tool, Workshop on Patent and Innovation Statistics, Organization for Economic Cooperation and Development, Paris, June 28 - 30, 1982.

annual percentage increase of patents from 1963 - 1972. Eight curves were fit to the points and the best fitting curve was used to make the projection.

Both of the above projections can be checked against subsequent data. The average percentage growth for the entire patent file was projected to be 2.2% for 1973 - 1977. My calculations based on actual data show that actual average percentage growth for this period was 1.8%. This may reflect the beginning of a decline in U.S. patent issues at that time. The average percentage foreign-origin for 1973 - 1977 was projected to be 33%; my calculation shows that the actual average percentage of foreign-origin was 34.6%, which may represent the beginning of the growth of foreign-origin U.S. patents. Figure 6 shows the actual and estimated data for the percentage of foreign-origin U.S. patents.

3. OTAF 7th Technology Assessment and Forecast Report

In the 7th Technology Assessment and Forecast Report, OTAF again attempted to forecast the number of patent issues, this time using data on patent applications, rather than patent issues, as the basis for the projection. It was anticipated that application data would result in more accurate projections because the number of patents that issue depends on the number of applications filed at an earlier date. Applications generally precede issues by an average of about two years. The application data had to be corrected for the average pendency, i.e., time required for a patent application to issue. Correction was also made for the abandonment rate, i.e., that proportion of patent applications which never issues, which varies by patent class.

Forecasts were made of the number of patent issues in 1976, 1977, and 1978, based on patented application data for the years 1967 - 1975 and the first half of 1976. Forecasts were made for six patent classes: Class 431, Combustion; Class 317, Electricity; Class 96, Photographic Chemistry, Processes and Materials; Class, 60, Power Plants; Class 427, Coating Processes; and Class 351, Optics, Eye Examining, Vision Testing, and Correction. Figure 7 shows graphs for three of the six patent classes.

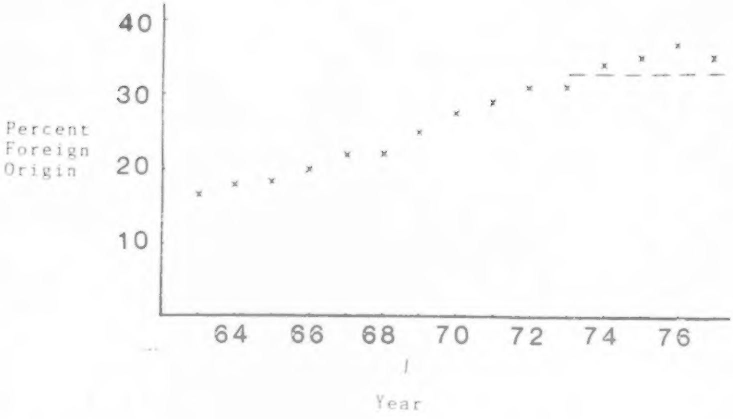
A subsequent check of the forecasts revealed that they varied in accuracy. Some were off by nearly 25%, while one was off by only about 1%. One of the problems in using patent application data at the class level is that when applications arrive at the patent office an initial class assignment is made, but is subject to change before the patent issues. Thus predictions made on the basis of the initial class assignment may be misleading.

4. OTAF Design Patents

Design patents are patents on the ornamental design of a manufactured article. In 1983 OTAF projected design patent activity using two different methods. First, percentage shares of design patent issues were projected for 1983, 1984, and 1985, based on the actual percentage shares for the years 1977 - 1982. Shares were projected for four categories of owners: U.S. corporate, U.S. individual, foreign corporate and foreign individual. Projections were

Figure 6

PROJECTED AVERAGE PERCENT FOREIGN-ORIGIN U.S. PATENTS 1973-1977
BASED ON PERCENT FOREIGN-ORIGIN U.S. PATENTS 1963-1972

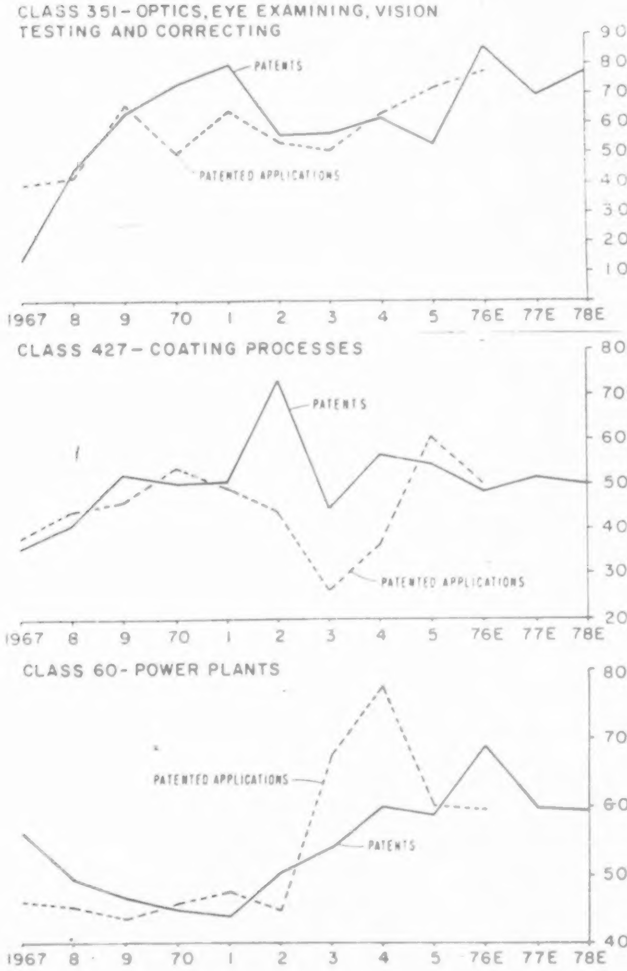


x = Actual Data
---- = Projected Average

Source: OTAF, Early Warning Report, December 1973

Figure 7

FORECASTING PATENT ACTIVITY BY USE OF
PENDING PATENT APPLICATION DATA
FOR THREE PATENT CLASSES



Source: OTAF, Technology Assessment and Forecast, 7th Report, March 1977 made using "an exponential smoothing function." The results are shown in Figure 8.

The second method projected levels of design patent issues for the years 1983 and 1984, based on application data for the years 1979 - 1982. Corrections were made for pendency and abandonment rates, as in the Seventh OTAF Report forecast. The results are shown in Figure 9.

Both projection methods showed that the percentage of foreign-origin design patenting would decrease in 1983 and stay below the 1982 level in 1984.

5. Martino, et al., Tests of Some Indices of Technological Change as Leading Indicators of Market Shift

Martino and colleagues examined three indices of technological change for their effectiveness as leading indicators of market shifts. The indicators were number of foreign-held U.S. patents, number of papers in technical and trade journals, and R&D expenditures. The study examined both shifts in import markets and shifts in domestic markets. Patents, however, were tested only for import market shifts -- i.e., shifts involving the substitution of a foreign-made product for a similar domestic product. The study was based on the premise that market shifts often result from technological change and in those cases it may be possible to anticipate the market shift by using leading indicators of technological change.

The approach was first to attempt to explain the observed market shifts with regression equations incorporating such variables as price, income, and tariffs, and then to add the technological indices to improve the explanatory power of the model. The analysis was conducted for 11 commodities.

The patent variable was a cumulative measure, rather than a simple time series, reflecting the idea that a patent is valuable for some period after the application is filed. Patents were treated as if their worth decayed 20% each year after the year of application. Six years after application a patent was counted as having no value. Five five-year-old patents equalled one "current" patent, i.e., one patent still within one year of application.[9] The patent data were obtained from OTAF.

Patents provided advance warning of import market shifts ranging from one to three years for three commodities: cameras, typewriters, and watches. In two cases, bicycles and television picture tubes, they did not provide a useful lead. In one case, motorcycles, patents lagged the market shift. Martino reported that a problem in using patents as leading indicators is that patenting practice varies from industry to industry because patents provide more effective protection in some industries. There were also problems in matching patent classifications to industry or product group classifications.

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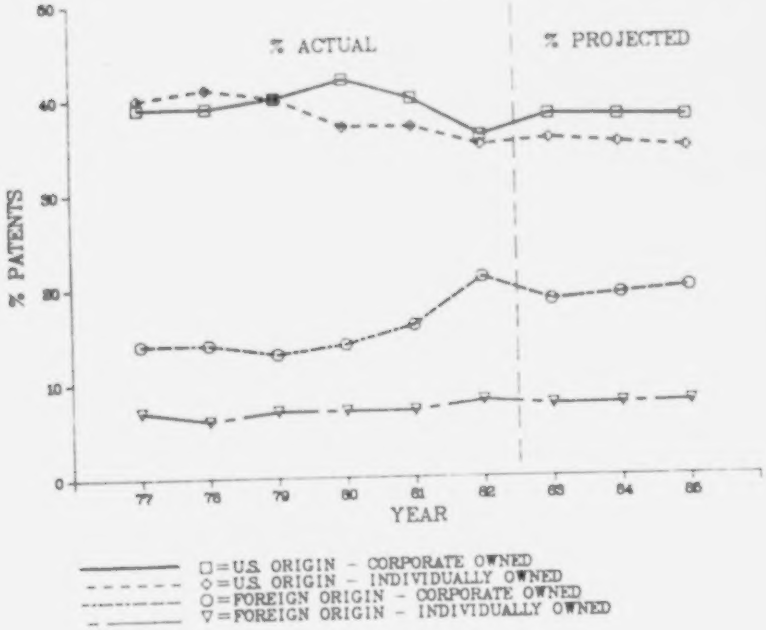
[9] Martino, et al., Tests of Some Indices, p. 2-12.

FIGURE 8

CURRENT AND PROJECTED DESIGN PATENT ACTIVITY
1977-1985



Source: OTAF, Design Patents, 1983

FIGURE 9

PROJECTIONS OF DESIGN PATENT GRANTS BASED ON NUMBER OF APPLICATIONS

Year	Total	U.S. Origin	Foreign Origin	% Foreign Origin
ACTUAL				
1981	4745	3570	1175	24.8
1982	4944	3466	1498	30.3
PROJECTED				
1983	4654	3269	1388	29.8
1984	5158	3651	1508	29.2

Source: OTAF, Design Patents, 1983

The study recommended additional research to determine the conditions under which patents are reliable leading indicators of market shifts. It recommended that cases should be selected where patents are both successful and unsuccessful as leading indicators of market shifts, for purposes of understanding the conditions under which they are reliable indicators and when they are not. The technical content of the patents could be compared to the technology incorporated into the new substitute products. Patents should also be investigated as a leading indicator for domestic market shifts, according to the report.

6. Martino, "Forecasting Technological Breakthroughs"

Forecasts based on past trends may seem inherently incapable of predicting technological breakthroughs -- which Martino defined as "an advance in the level of performance of some class of devices or techniques that significantly transcends the limitations of prior devices"[10] -- because breakthroughs seem to imply lack of advance warning. Martino argued, however, that most breakthroughs are preceded by precursor events and that it is possible to forecast breakthroughs by systematically searching for precursors and evaluating them.

Martino's hypothetical example, of an airplane engine manufacturer monitoring for breakthroughs in airplane engines in 1930, used a key patent as a source of information in the data collection phase. The patent was a British patent to Frank Whittle for an aircraft engine based on the jet principle, which if it became practical could threaten the manufacturer's product line. The patent, once identified, raised a series of questions which when formulated could provide the basis for a monitoring system. In this case, the idea for a jet engine was not new; in fact previous patents had been awarded for similar engines. The reason these inventions had not yet been implemented was the existence of several technical constraints. However, work was going on to address those technical problems. Thus the job of the monitor would be one of monitoring progress on these critical technical problems and determining the threshold--that is, the point at which the jet engine posed an actual threat to the company.

This application of patents for forecasting does not rely on the statistical properties of patents, but on the unique information contained in an individual patent. The information that is relevant to the forecast is the nature of the technological problem being addressed and the proposed solution, the fact that it was deemed important enough to have been patented, who made the invention, and who owned the patent. The patent is just one part of the monitoring system. The technical problem that it identifies is one that in the opinion of the inventor can be solved and for which there is some reward for solving. The patent functions mainly by raising questions which require technological competence to formulate and to answer.

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[10] Martino, p. 129.

7. Hack and Matejka, "Prognosis of the Development of Artificial Kidneys Based Upon Patent Literature"

This article is interesting because of its methodology and because it actually makes forecasts.[11] It describes a methodology for classifying and sorting information from patent specifications by key words, breaking that information down into a multi-level hierarchy and then resynthesizing the information back up the hierarchy to reach "prognostic considerations." The process is reportedly laborious and time-consuming, but deemed by the authors to be very valuable. The results, they said, were almost identical with those published in scientific papers and in commercial and publicity literature. The patent literature provided greater technical detail and allowed the authors to assess the state of technologies some five to ten years ahead of the state of products on the market.

The authors assembled a large number of patents covering the period 1964-1978, based on the IPC classes covering the subject of failing kidney replacement. The following information was recorded for each patent: basic identifying data -- e.g., country, number, priority, applicant; characteristics of the invention in terms of key words; state of the technology at the time of filing; disadvantages of the given state of technology; embodiment with respect to the claims.

In the first three steps of a ten-step procedure, the patents were sorted into categories and subcategories of common features of function and design. The patents fell into the following five categories:

sorbents used to remove metabolic waste
diaphragms for haemodialyzers
haemodialyzers
monitoring equipment
future means of renal failure replacement

The authors found that this breakdown corresponded to the structure of the market for hemodialysis devices.

The rest of the procedure is diagrammed in Figure 10. In steps 4, 5, and 6, each patent was analyzed for the state of the technology at the time of filing, the technical problems the inventor was trying to correct, and the nature of the solution, respectively. These steps resulted in a "classification tree," in which key words are arrayed by classification level. (See Figure 11.)

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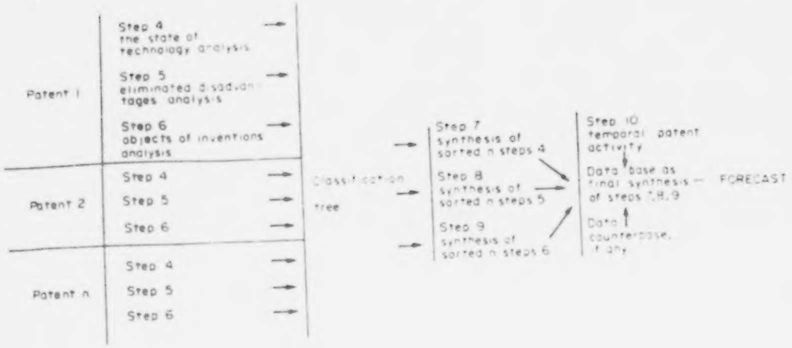
[11] Although the article is in English, it appears to be a translation from another language and is difficult to follow in some places.

Figure 10

FLOW CHART OF FORECAST PROCESS
FROM HACK AND MATEJKA


Patent 1
Step 4
the state of technology analysis
Step 5
eliminated disadvantages analysis
Step 6
objects of inventions analysis
Patent 2
Step 4
Step 5
Step 6
Patent n
Step 4
Step 5
Step 6
Classification tree
Step 7
synthesis of sorted n steps 4
Step 8
synthesis of sorted n steps 5
Step 9
synthesis of sorted n steps 6
Step 10
temporal patent activity
Data base as final synthesis of steps 7,8,9
FORECAST
Data counterbase, if any

Source: Rudolf Hack and Libor Matejka, "Prognosis of the Development of Artificial Kidneys Based Upon Patent Literature," World Patent Information, v. 4, 1982: 23 - 32.

Figure 11

CLASSIFICATION TREE OF RENAL REPLACEMENT DEVICES
BASED ON KEY WORDS IN PATENTS

Classification level	I	II	III	IV	V	VI
Key words	Sorbents in renal replacement	adsorbents for purification of liquids	dialyzate blood plasma filtrate			
		adsorbents as membrane supports				
		adsorbent grafted membranes	on surface inside			
	Membranes	improvement of existing membranes	plate sleeve hollow fibre			
		new materials	plate sleeve hollow fibre			
		adsorbent grafted membranes	plate sleeve hollow fibre inside outside			
	Haemodialyzers	non-disposable	coil	sleeve plate		
			plate	Kiil modular envelope		
		disposable	coil	sleeve plate		
			parallel flow	hollow fibre plate	plate sleeve Z-folded	art of sealing
	Monitoring equipment	extracorporal blood circuit	cleaning and sterilization monitoring the blood level ultrafiltrate rate control circulation access devices			
		dialyzate fluid circuit	central delivery dialyzate mixing dialyzate degassing dialyzate recirculation ultrafiltrate measurement and control cleaning and sterilization			
	Unconventional means for renal replacement	parabiotical device kidney and liver replacement plasmapheresis preserving organs device used as renal replacement				

Source: Rudolf Hack and Libor Matejka, "Prognosis of the Development of Artificial Kidneys Based Upon Patent Literature," World Patent Information, v. 4, 1982: 23 - 32.

In steps 7, 8, and 9 the base of information created by the previous three steps was used in conjunction with the classification tree to synthesize the history and state of the technology, technical problems addressed, and nature of the solutions, respectively. These syntheses then formed the base for the forecast.

In step 10, the synthetical statements on the state of technology, technical problems addressed and nature of solutions were compared to the current state of the technology as represented in catalogs and leaflets of commercial literature. This information was also compared to patent activity, assuming that "when an object shows a steep increase [in patent activity] right from the beginning, it doubtlessly means that a solution will be found in the near or distant future...."[12]

From this base the authors made a series of forecasts of the function and design of future artificial kidney technology. Examples are provided of the forecasts and notes on how they were developed from the materials described above. The forecasts do not specify a time when the forecasts will be realized or the probability of their occurence.

8. Mlodzik, "Patent Literature--A Tool for Forecasting...?"

Mlodzik used the DERWENT CPI (Chemical Patent Index) file, which includes information on international patent families, to analyze trends in patenting activity in the field of antirheumatics. For five major pharmaceutical companies, she compared trends in patenting to trends in market share and concluded that there were some similarities in rankings and trends in patent activity and market share. Then the author looked at all pharmaceutical patenting by a larger number of firms, as well as their patenting in the antirheumatic field. She interpreted the total number of pharmaceutical patents held by a corporation as an indication of its "innovation potential," a term for which she provided no definition.

She then showed how the important inventions could be identified by counting the number of members in the international patent family. Multinational corporations often seek to patent an invention in several countries. In such cases, the patents on a single invention in different countries are termed an international patent family. Generally the first patent application in a family is termed the "priority application" or basic patent. By counting priority applications, it is possible to count the number of unique inventions. Mlodzik argued that more important inventions are patented in more countries -- i.e., have more international equivalents -- than less important inventions. Figure 12 shows the number of basic patents and equivalents for four companies. It shows different patterns in both the number of basic patents and in the tendency to obtain equivalent patents in multiple countries. For example, the Japanese firm, Yoshitomi, registered most of its inventions in

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[12] Hack and Matejka, p. 28.

Figure 12

INTERNATIONAL PATENT FAMILIES: NUMBER OF BASIC PATENTS AND FOREIGN EQUIVALENTS FOR FOUR COMPANIES

MERCK		YOSHITOMI		CIBA-GEIGY		RHONE-POULENC	
Patents	Equivalents	Patents	Equivalents	Patents	Equivalents	Patents	Equivalents
1	1	1	1	1	20	1	34
2	2	2	1	2	1	2	13
3	1	3	2	3	1	3	9
4	1	4	1	4	24		
5	9	5	20	5	1		
6	1	6	1	6	7		
7	1	7	1	7	1		
8	14	8	1	8	14		
9	1	9	1	9	0		
10	1	10	1	10	12		
11	8	11	1	11	1		
12	0	12	1				
13	6	13	3				
14	18	14	1				
15	1	15	1				
16	4	16	1				
17	7	17	2				
18	6	18	2				
19	1	19	1				
20	12	20	1				
21	6	21	1				
22	1	22	1				
23	1	23	.				
24	2	.	.				
25	4	.	.				
26	7	.	.				
27	7	.	.				
28	7	.	.				
29	0	39	1				
30	15	40	14				

Source: Hanna Mlodzik, "Patent Literature -- A Tool for Forecasting in the Pharmaceutical Industry?"

Japan only, while Ciba-Geigy and Rhone-Poulenc both showed a tendency to register fewer unique inventions but to obtain equivalents in more countries. It is not clear whether this is the result of patent policies or of less research work in these companies.

The author suggested that by reviewing the chemical structures in the important inventions identified in this way, one may make a rough prediction of drugs that will be on the market in the future. In a statement notable for its vagueness, she concluded that "Among these structures we will find the new drugs of the future....[P]robably we will find a few of them on the market as new drugs in approximately 10 years."

9. Faust, "Patentdaten als Frühindikatoren"

The work of Faust, reported in "Patentdaten als Frühindikatoren der Technologischen Position Konkurrierender Industrieländer" (Patent data as early indicators of the technological position of competing industrialized countries), was aimed at predicting future international trade competition on the basis of the technological positions of individual countries. Most of the article is devoted to showing how patent statistics may be used to measure and compare national technological positions.

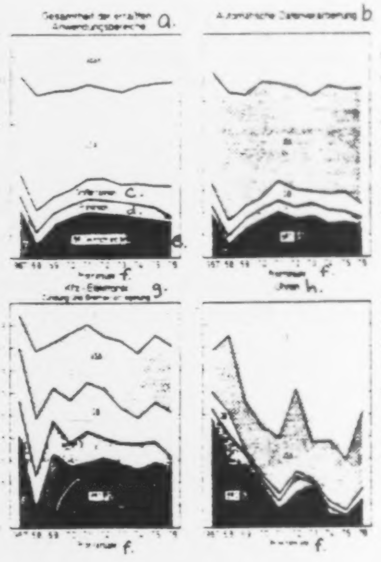
He used INPADOC data on international patenting in the International Patent Classification (IPC) classes corresponding to microelectronics. INPADOC data include information on international "patent families." First, by counting priority applications, Faust looked at the number of patented inventions originating in each of five countries -- the Federal Republic of Germany, France, the United Kingdom, Japan, and the United States.

Then he looked at the number of foreign patent applications corresponding to those inventions and the percent of those foreign patent applications held by each of the five countries. Figure 13 from Faust shows trends in the shares of these five countries in foreign patent applications for the overall field of microelectronics, as well as the subfields of automatic data processing, clocks, and motor vehicle electronics -- ignition and braking force adjustment -- from 1967 - 1976. These graphs allow one to make empirically based statements about the trends and current status of relative national technological positions. Faust did not actually make a projection, but by implication this would be done by extrapolating the trends in the graphs.

The author noted that patent statistics can only be early indicators for national competitive position if the "technology-push" model of innovation holds, rather than the "demand-pull" model. Basically these are models that specify opposite causal relationships between technological change and economic growth. In the technology- push model, technological change causes subsequent economic growth. In the demand-pull model, economic demand stimulates the development and application of new technology. Faust found that the technology-push model seemed to apply for some microelectronics product groups and the demand-pull model for others. In the latter product groups international competitive position was affected primarily by such factors as cost, prices, and trade barriers. Even in such product groups, Faust argued, it is important to

Figure 13

FOREIGN PATENT APPLICATIONS IN THE FIELD OF MICROELECTRONICS
SHARES OF SELECTED COUNTRIES IN PERCENTAGES



Key:

a. Total included
fields of application

b. Automatic data processing

c. Great Britain

d. France

e. Federal Republic of
Germany

f. Year of priority

g. Motor vehicle electronics
--ignition and braking
force adjustment

h. Clocks

Source: Konrad Faust, "Patentdaten als Frühindikatoren der technologischen Position konkurrierender Industrieländer," IFO-Schnelldienst, 1981.

monitor the technological positions of competitors because even if innovation potential is limited, "one can secure a certain assurance of market position with improvement innovations."[13]

10. Summary

Five of the works reviewed actually made forecasts -- the three OTAF reports, Hack and Matejka, and Martino, et al. The OTAF reports forecast future numbers of issued patents based in one method on past numbers of issued patents and in the other method on past numbers of patent applications. The OTAF forecasts drew on the OTAF data base and utilized various means of statistically projecting past trends. The time frame of the forecasts was specified.

Hack and Matejka forecast future technical functions and designs in the area of kidney replacement, based on consideration of both the technical information disclosed in the patents and patent counts. The procedure employed was difficult and time-consuming. However, the authors report that the results of their forecast correspond closely to that obtained from experts and the scientific literature. The time frame of the Hack and Matejka study was not specified.

Martino and his colleagues examined the effectiveness of patents as a leading indicator of import market shifts by conducting retrospective predictions. They entered lagged patent data into a regression equation with other economic variables in hopes of improving the model's ability to explain observed market shifts. They found that patents were successful leading indicators for some of the 11 commodities studied, but not for others.

The remaining papers did not attempt explicit forecasts. Faust, Mlodzik, and Campbell used patent data in a variety of ways to make statements about, as Faust expressed it, the development of the current technological position. Faust's ultimate goal was to predict future development of international competition among certain industries in industrialized nations. Such a forecast would be based on statements about the development of those nations' current technological positions. Faust showed how international patent family statistics from INPADOC can be used to develop such statements. He mentioned some of the assumptions necessary to make predictions on the basis of such data but did not make any predictions.

Mlodzik's ultimate objective was to forecast future new pharmaceutical products in the field of antirheumatics. She showed how international patent family data from Derwent can be correlated with firm market share and used to compare corporate technology strategies. She suggested, but did not illustrate, that patent family information can be used to identify "important" pharmaceutical inventions, whose chemical structures can then be studied. She suggested, rather imprecisely, that one could expect a few of these inventions

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[13] Faust, p. 22.

INTENTIONAL
BLANK

to be on the market in about ten years. Mlodzik's approach involved both patent counts and the technological information disclosed in the patents.

Campbell's goal was to provide a decision making tool for companies that would allow them to evaluate their technological position and that of their competitors. He used both quantitative and qualitative data, manually extracted from individual patents, to develop a set of dimensions describing a field of technology. He implied that certain patterns in these dimensions could be used to forecast the future development of a field of technology or competitors' actions. He stopped short of making such forecasts in the reviewed reports.

Martino, in his book on technological forecasting, described a model system for monitoring for technological breakthroughs that could use patents as an information source. The system would function to identify precursor events and use them to provide advance warning of breakthroughs that might pose a competitive threat. Martino's example used the technical information in the patents.

The reviewed literature reveals that there is not a well-developed method of applying patent data in making forecasts. Forecasts were few in number, not particularly sophisticated in terms of forecasting methodology, and of unknown validity. They were interesting in that they revealed two different approaches to using patent data. One is statistical analysis of computerized data routinely recorded for all patents by the patent office and the other is analysis of technical information manually extracted from individual patents. The information used in the latter approach is much richer, but much more difficult to obtain and use.

The articles that did not attempt explicit forecasts for the most part explored ideas for how patent data might be used in various kinds of technical and commercial analyses. An important purpose of these articles was to inform researchers and others of the availability of patent data. With few exceptions, the studies failed to examine the conceptual underpinnings and assumptions underlying the use of patents to measure technological activity.

While use of patent data in forecasting is not well-developed, patents are being used increasingly for other purposes. The next section reviews some of the ways patent data are being used today.

III. OTHER USES BEING MADE OF PATENT DATA

In the approximately 15 years that patent data have been more available, they have been used increasingly for research and analysis on issues of science and technology policy and strategy. Quite an extensive literature has developed on patent statistics as indicators of innovative activity, which Pavitt has recently reviewed.[14] To provide an overview of this field, it is useful to look at it from three perspectives: the users, the approaches, and the levels of analysis. I shall draw heavily on Pavitt's review in discussing the latter two perspectives.

1. Users

Patent data are being used by firms, governments, and by researchers. Firms are using patent data to obtain information on their technology holdings, their patterns of strength and weakness, and those of other firms, including competitors or potential competitors. They are using them also to evaluate trends in particular fields of technology, for example, fields in which they are active or are considering entry.

Governments use patent data as a tool in evaluating the health of national science and technology systems and making related policy decisions. Patent data are also used in studies of the international competitiveness of national industries.

Researchers such as economists and technology planners have been using patents in a variety of ways to study the process of technological innovation. Their work in a sense underlies that of other users because researchers are often pioneers in new data applications, contributing to methodology development and conceptual validation.

2. Approaches

Pavitt distinguished three general analytical approaches to patent data in the context of science and technology policy. One is monitoring trends in patenting activity at fairly aggregate levels. This is the approach of Science Indicators, a government publication that presents patenting statistics along with other indicators of scientific and technological activity for the purpose of monitoring trends and making comparisons among countries and industries.[15]

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[14] K. Pavitt, "Patent Statistics as Indicators of Innovative Activities: Possibilities and Problems," Scientometrics, v. 7, January 1985: 77 - 99.

[15] See for example, U.S. National Science Board, Science Indicators 1982 (Washington,D.C. : U.S. Govt. Print. Off., 1983).

The second approach uses patent citations and co-citations to trace relationships between fields of technology, between science and technology, and between the technical activities of different firms and different countries. This approach is exemplified by Computer Horizons, Inc., and to some extent by Battelle Pacific Northwest Laboratories.[16]

In the third approach patent statistics are used to explore causal relationships between patent activity and other measured economic variables to test theories and models about determinants and consequences of technological activity. It is exemplified by the work of economists such as Griliches and his colleagues at the National Bureau of Economic Research, as well as Scherer, and Soete.[17]

3. Levels of Analysis

Patents are being used to conduct studies at several different levels of analysis. Pavitt described the use of patent statistics to make comparisons at the level of technologies, firms, industrial sectors, nations and over time. The following discussion is drawn largely from Pavitt's review.[18]

A. Comparisons Among Technical Fields

Patents are assigned to technologically based patent classes that can be compared to each other. Using this classification, OTAF published a series of reports that made possible the comparison of trends in the level of patenting, the national origin of patenting, and the ownership of patents across technologies. OTAF also periodically published statistics on rapidly growing patent classes and subclasses, called "Active Technological Areas." The assumption behind these reports is that changes in the growth of patenting in particular patent classes reflect changes in the rate of innovation in specific technological fields. Pavitt pointed out, however, that this assumption has not been rigorously tested. He argued that these data should be tested against expert opinion and historical experience to see if they reflect innovative activity.[19]

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[16] See for example, M.P. Carpenter, F. Narin, and P. Woolf, "Citation Rates to Technologically Important Patents," World Patent Information, v. 3 (1981): 160-163; also Campbell, op. cit.

[17] Z. Griliches, ed. R&D, Patents, and Productivity (Chicago: University of Chicago Press, 1984); F.M. Scherer, "The Propensity to Patent," International Journal of Industrial Organization, v. 1 (1983): 107-128; L. Soete, "A General Test of Technological Gap Trade Theory," Weltwirtschaftliche Archiv, v. 117 (1981): 638 - 660.

[18] Pavitt, "Patent Statistics."

[19] Pavitt, p. 89.

Walsh has done some work along these lines in a study using patent statistics, along with statistics on scientific papers and economic activity, to plot the development of major new product fields in the chemical industry since the mid-19th century. Patent data revealed rapid rates of growth in synthetic dyes after 1880, and in plastics and pharmaceuticals after the mid 1920s. Walsh found, however, that "the volume of patent statistics in different categories does reflect differences in 'secondary and induced innovative activity,' but cannot detect the appearance of major or radical innovations which lead to rapid growth." She concluded that patent statistics must be interpreted together with a qualitative knowledge of the technology.[20]

Computer Horizons, Inc. (CHI) have tried to determine the degree of technological advance represented by patents by drawing on bibliometric techniques. They found that the patents related to a sample of significant innovations identified by experts were more highly cited than a control group of randomly selected patents.[21] They have also used patent citation data to look at the links between particular fields of technology and their underlying science bases. They argue that if U.S. patents in a field of technology cite scientific papers to a relatively high degree, then that field is closely linked to science. In biotechnology they found such links to academic science and in electronics they found links to industrial science.[22]

B. Comparisons Among Industrial Firms

Several researchers have studied the relationship between R&D and patenting within firms. Soete,[23] and Pakes and Griliches[24] have found positive and statistically significant relationships between cross-sectional variations in firms' R&D and patenting activities. Of the two studies, which were based on different samples, Soete's correlations were lower. There are reasons why one might not expect high correlations between firms' R&D and patenting. One is the highly skew distribution of the economic value of patents and the uncertainty as to the value of the next patent on the margin.[25] This high uncertainty for the majority of patents whose value is expected to be relatively small may result in a situation where firms that have the same volume of innovative activity patent different amounts of output. The second reason is that, as Schmookler, Soete, and Scherer have all shown, there is a declining propensity to patent as the size of the R&D program or the firm increases.[26] Pavitt suggested that at the lower end this may reflect the growing imperfection of R&D expenditures as a proxy for innovation. At the upper end it may reflect the relatively strong participation of firms in sectors with characteristics associated with relatively low propensity to patent as described by Scherer -- i.e., high industrial concentration and high levels of government R&D funding. Such firms may have more effective barriers to imitation than patents, such as natural imitation lags.

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[20] V. Walsh, "Invention and Innovation in the Chemical Industry: Demand-Pull or Discovery-Push?" Research Policy, v. 13 (1984): 211 - 234.

[21] Carpenter, Narin, and Woolf, op cit.

[22] M. Carpenter, M. Cooper, and F. Narin, "Linkage Between Basic Research Literature and Patents," Research Management, v. 13 (March 1980): 30 - 35.

[23] L. Soete, Inventive Activity, Industrial Organisation, and International Trade, (D. Phil. Thesis, University of Sussex, 1978).

[24] A. Pakes and Z. Griliches, "Patents and R&D at the Firm Level: A First Look," in Griliches, ed., op. cit., pp.

[25] A. Pakes and M. Schankerman, "The Role of Obvolescence of Knowledge, Research Gestation Lags and the Private Rate of Return to Research Resources," in Griliches, ed., op. cit.

Griliches et al. have analyzed time-dependent relationships between R&D and patenting in firms, that is, the extent to which changes in R&D expenditures are related to subsequent changes in patent activity. They found statistically significant, positive relationships between variations in R&D and in patenting, but they have not yet found any statistically significant lagged relationship between R&D and patents. Changes over time in the volume of firms' R&D are reflected almost immediately in changes in the volume of patenting.[27] Pavitt argued that these findings are not consistent with linear models wherein R&D leads to invention and invention leads to innovation, but rather support the assumption that R&D and patenting are both proxies for firms' innovative activities.

OTAF has shown that U.S. patent data can be used to estimate the international distribution of innovative activity within large multinational corporations -- based on the residence of the inventor.[28] It is not known with certainty that residence of inventor reflects accurately the country where the R&D was done, however.

Patent statistics categorized by patent classification can provide technological profiles of specific industrial firms, that is, the firms' distribution of patenting activity among patent classes. CHI and Battelle have shown how these data can be used by firms to (1) reveal their relative technical strengths and weaknesses, (2) compare their innovative activity with those of other firms, and (3) through analysis of networks of citations among patents, to identify technical complementarities and competition among patenting firms.[29] Jaffe has estimated the "technological proximity" of firms, defined as the similarity of their profiles.[30] Dosi and Pavitt are testing the hypothesis that firms with similar primary activities have similar technological profiles, that the shape and distribution of such profiles vary among sectors of principal activity, and that they can be categorized and predicted.[31]

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[26] Schmookler, op. cit.; Soete, op. cit.; Scherer, op. cit.

[27] Pakes and Griliches, op. cit.

[28] Office of Technology Assessment and Forecast, "Patent Trends: Foreign Multinational Corporation Patenting Trends in the U.S." Tenth Technology Assessment and Forecast Report (Washington, D.C.: U.S. Govt. Print. Off., November 1981) pp. 33-46.

[29] F. Narin, M. Carpenter, P. Woolf, "Technological Performance Assessments Based on Patents and Patent Citations," IEEE Transactions on Engineering Management, v. 4 (November 1984): 172-183.

[30] A. Jaffe, Using Patent Class Data to Measure Technological Proximity and Research Spillovers Among Firms (Cambridge, MA: National Bureau of Economic Research, Summer Institute Paper, August 1983).

C. Comparisons Among Industrial Sectors.

Pavitt described two uses for data on the distribution of patenting activity among industrial sectors, although he noted that there is a problem in establishing a satisfactory correspondence between patent classes and industrial classes. The first is to enable statistical testing of models or theories of relations between innovative activities and economic and social variables. Soete used U.S. patent statistics broken into 40 industrial sectors to test the "technology gap" theory of international trade by regressing countries' shares of total OECD exports in each sector against their shares of patenting in the United States, among other variables. The relationship was statistically significant at the 5% level in 28 of 40 sectors.[32] Scherer and his colleagues manually categorized 15,000 patents by industry of origin and industry of use to trace intersectoral flows of technology in the United States and to test Schmookler's hypothesis that intersectoral differences in the volume of innovative activity reflect sectoral differences in the volume of investment.[33]

The second use of patent statistics in studies of industrial sectors is to compare the volume of innovative activity in different sectors. The relatively poor correlations between R&D and patenting in some industrial sectors affect this use also. Nonetheless, the distribution of patenting and R&D across industrial sectors in both the United States and the United Kingdom show the same characteristics: high shares of both patenting and R&D in chemicals, electric and electronic products; much higher share of R&D than of patenting in motor vehicles and aerospace; and much lower shares of R&D than patents in mechanical engineering, instruments, and fabricated metal products.[34] Discrepencies in the sectoral shares reflect both the difficulty of measuring and sectoral attribution of innovative activity and sectoral differences in the propensity to patent the results of innovation.

Scherer found in a detailed matching of 15,000 patents to the company line-of-business R&D that gave rise to them that the ratio of patents to R&D for mechanical engineering, instruments, and metal products was only slightly above average. But he found the same relatively low level of patenting compared to R&D in aircraft and automobiles -- less than half the average for all sectors.

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[31] Pavitt, op. cit., p. 90.

[32] L. Soete, "A General Test of Technological Gap Trade Theory."

[33] F.M. Scherer, "Demand Pull and Technological Invention: Schmookler Revisited," Journal of Industrial Economics, v. 30 (1982):

[34] K. Pavitt, "R&D, Patenting, and Innovative Activities: A Statistical Exploration," Research Policy, v. 11 (1982): 33 - 51; J. Townsend, et al., Innovations in Britain Since 1945, (Sussex, U.K.: University of Sussex, Science Policy Research Unit, Occupational Paper No. 16, 1981).

For more than 4000 lines of business, the r2 was about .49.[35] Scherer's work suggests that, when properly matched at a fine level of detail, sectoral shares of R&D and patenting are closely correlated. Scherer's method, however, was highly labor-intensive and not easily or routinely reproduced.

D. International Comparisons

A number of studies have been done, based on the assumption that national differences in the volume of domestic patent activity reflects national differences in the volume of innovative activity. Pavitt[36] and Soete[37] have found statistically significant relations between national per capita expenditures on industrial R&D and national per capita patenting. They did not distinguish, however, between patents to residents and patents to nonresidents, which are now a significant proportion of total patenting in most industrialized countries.

International differences in the volume of patenting in a given foreign country -- e.g., differences in the level of Japanese and West German patenting in the United States -- may be a more reliable measure of international differences in innovative activity. In such comparisons the patenting is all subject to the patent law of the host country, thus eliminating the effects of different national patent laws. Such comparisons also tend to equalize the costs of the patenting being compared.

There is evidence of strong statistical relationships between international differences in the volume of industrial R&D activity and the volume of patenting activities in third countries. Soete and Wyatt found a statistically significant relationship between per capita expenditures on industrial R&D in each OECD country and its per capita patenting in the United States, Japan, the Federal Republic of Germany, and the United Kingdom. (The home country was excluded from the calculations.) R2 was greater than .83 in each case.[38]

Pavitt noted that foreign patenting measures have been criticized because of possible systematic, country-specific biases in propensity to patent the output of innovation in foreign countries. The propensity may be strongly influenced by the actual or expected market share of the innovating firm in the foreign country. Soete and Wyatt found instances when foreign patenting was higher than expected based on industrial R&D -- for example, Canadian patenting in the United States, Danish and Austrian patenting in the Federal Republic of Germany; Australian, Irish, and New Zealand in the United Kingdom. This is probably due to close trading relations.[39] Pinson showed considerable variations among countries in the international distribution of patenting activities. For example in 1970 about 37% of all Canadian foreign patenting was in the United States compared to 27% for Japan, and 9 - 15% for Western Europe.[40]

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[35] Scherer, "Propensity to Patent."

[36] Pavitt, "R&D, Patenting, and Innovative Activities."

[37] Soete, Inventive Activity, Industrial Organisation, and International Trade.

[38] Soete and Wyatt, op cit.

[39] Ibid.

Pavitt recommended continuing the use of third country patenting studies because "behind country specific biases introduced by trading relationships, patenting in foreign countries does reflect on-going innovative activities, as well as innovators' perceptions of benefits from exploiting the results of those activities in foreign markets."[41] The high correlation between industrial R&D and foreign patenting suggests that at least in larger countries with big potential markets for innovations, foreign patenting reflects innovative activity. This is confirmed by Basberg who showed that the sectoral patterns of Norwegian patenting in the United States reflected not the patterns of Norwegian exports to the United States, but the sectoral pattern of Norwegian innovation.[42]

Systematic differences among countries in the propensity to patent a given output in foreign countries may also reflect national differences in patenting practices. For example, the volume of eastern European and USSR foreign patenting does not reflect their R&D activities. There is debate on whether such differences exist among Western industrialized countries.[43]

5. Comparisons Over Time

One of the primary uses of patent data has been to monitor changes over time. Such studies have led to two notable findings. One is the series of OTAF reports showing the increasing share of U.S. patenting over time which is of foreign origin. The other is the declining ratios of patenting to R&D over time in most industries except pharmaceuticals and some other chemical sectors found by Soete[44] and by Pakes and Griliches.[45] The first finding has led some analysts to conclude that the U.S. share of world innovative activity is declining relative to competitors. Others, such as Soete, Evenson, and Hausman et al., have concluded from the second finding that R&D has reached diminishing returns in terms of its output of inventions and innovations.[46]

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[40] M. Pinson, Trends in Numbers of Applications for Patents According to Various Criteria in Certain OECD Member Countries (1950 - 1980). Workshop on Patent and Innovation Statistics, (Paris: Organization for Economic Cooperation and Development, June 28 - 30, 1982).

[41] Pavitt, "Patent Statistics," p. 84.

[42] B. Basberg, "Foreign Patenting in the USA as a Technology Indicator: The Case of Norway," Research Policy, v. 12 (1983):

[43] Pavitt, "Patent Statistics," p. 85.

[44] Soete, Inventive Activity.

[45] Pakes and Griliches, op. cit.

[46] Soete, Inventive Activity; R. Evenson, "International Invention: Implications for Technology Market Analysis," in Griliches, ed., op cit., pp. 89 - 126.; Hausman, et al., "Econometric Models for Count Data with Applications to the Patents-R&D Relationship, Econometrics, 1984.

Pavitt argues that such conclusions are premature. For one thing, in most industrialized countries the share of patenting of foreign-origin has been increasing. Schiffel and Kitti concluded that the trend reflects increasing economic interdependency and the growing proportion of each country's sales in foreign markets.[47] Second, it is difficult to explain why diminishing returns would happen simultaneously in a wide range of disparate economic and technological sectors. Such a phenomenon seems inconsistent with the continued increases in firms' R&D (in real terms) that have occurred.

Soete has suggested that declining patents per R&D ratios reflect increasing industrial concentration.[48] Other explanations could be increasing costs of patenting and increases in relative efficacy of other forms of protection. Another explanation could be that although numbers of patents are decreasing, this is being offset by increases in the quality or value of patents. Schankerman and Pakes[49] have provided some evidence for this based on analysis of patent renewals in Europe. There is little direct behavioral evidence from firms about the reasons for increasing foreign patenting or declining patents per R&D, although some work is in progress.[50]

Pavitt has shown that national rankings in the rate of increase of R&D in firms are almost the same as those in the rate of increase in foreign patenting activity in the United States. This suggests that the increase in the propensity to patent the results of R&D in foreign markets has been increasing at about the same rate in all OECD countries. Thus, he argued, changing national shares of foreign patenting over time will be an accurate reflection of changing national shares of innovative activities.

Pavitt's review of the use of patent statistics as indicators of innovative activities reveals that an extensive amount of work has been done and that patent data are being used in a variety of often imaginative ways. His review, however, addressed the problems of patent statistics, as well as their potential. In fact, no paper on the use of patents in analysis would be complete without a discussion of data problems. These issues are elaborated upon in the next section.

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[47] D. Schiffel and C. Kitti, "Rates of Invention: International Patent Comparisons," Research Policy, v. 7 (1978):

[48] Soete, Inventive Activity.

[49] M. Schankerman and A. Pakes, "Estimates of the Value of Patent Rights in European Countries During the Post-1950 Period," (Cambridge, MA: National Bureau of Economic Research, May 1985).

[50] Pavitt, "Patent Statistics, p. 93."

IV. PROBLEMS WITH PATENT DATA

Patents have long been subject to criticism as a source of data for statistical analysis. These criticisms are pertinent to the use of patents in technology forecasting because such use is premised on the assumption that patents are an adequate measure of trends in innovative activity on which to base projections of future activity. In this section I summarize the pros and cons of patent data for analysis of innovative activity.

On the pro side, I have already pointed out that patents are one of the few direct, quantitative reflections of technological activity available. Patent data are available for many, if not all technologies, many countries, and long time series. They contain detailed information on the technology, the inventor, the owner; the dates of application, issue, and foreign priority; and prior art. With advances in information technology and management, these data lend themselves increasingly to analysis of trends over time, and comparisons across technologies, companies, industries, and countries. Patent statistics have great potential for improving our understanding of technological change and its causes and consequences. Therefore it is time to reassess the shortcomings of patent data and to devise methods of correcting those problems or adjusting for them to the extent possible.

The key criticisms of patent data are: (1) patent counts are not an adequate measure of innovative activity, (2) systematic bias exists in patent statistics, and (3) there are problems in relating patent statistics to other data because of incompatible classification systems.

1. Adequacy of Patent Statistics as Indicators of Innovative Activity

Just what do patent counts measure? It is really more correct to speak of patent statistics as "indicators," rather than "measures." Indicators are statistics that seek to quantify a phenomenon that cannot be measured directly. Technology, as opposed to its embodiments in physical devices, is a form of knowledge. Technological advance is not observable but can only be inferred from activity associated with the production of new technological knowledge -- e.g., R&D expenditures and patent counts.

There is some disagreement about what patents indicate. This is due to some extent to fuzziness in the underlying definitions and concepts of technology, invention, and innovation, and our models of how these things are related to each other and to other phenomena of interest such as R&D activity and economic growth. The sometimes implicit model underlying much discussion of innovation indicators is as follows:

Innovative Activity

R&D----►Invention---------►Innovation-----►Economic Consequences
(commercial
introduction)

-------TIME-----►

In this model, invention is a step between R&D and commercialization of an innovation, which in turn precedes economic effects of the innovation. If patents are taken as an indicator of invention, then they should provide some "early warning" about innovation and economic effects. They should also provide a tool for evaluating the output of R&D.

It should be noted that the linear progression-of-stages model above has been thoroughly discredited as a representation of reality. It has been pointed out that the stages of the innovation process are not so neatly distinguishable as implied, that there is not a simple and easy progression from stage to stage, that not all inventions and innovations result from R&D, and that there are important causal relationships in the opposite direction, e.g., economic demand often "pulls" R&D and invention.[51] Yet it continues to influence thought in this area because it is easy to think about and superficially useful for organizing data. Moreover, no superior model has been proposed that has those characteristics.

The basic debate about what patents indicate is whether they represent invention, i.e. the addition to technological knowledge that precedes commercial introduction, or whether they represent innovative activity more broadly, including the commercial and economic consequences of additions to technological knowledge.

In an earlier report I held that patents are, strictly speaking, an indicator of invention, not innovation, because by law patents are granted for inventions.[52] A patented invention does not have to be commercially introduced. In fact a significant proportion never are.[53] In practice, however, it appears impossible to separate the purely technological dimension of patents from their economic dimension. The decision to apply for a patent, at least among private firms, is an economic one. Industrial firms patent primarily to protect innovations from imitation by competitors. Thus whether a particular invention is the subject of a patent application depends in part on whether the firm intends to compete in that market and on the attractiveness of patent protection compared to alternative methods of protecting innovations, such as trade secrets or natural imitation lags. Also there is likely to be considerable variation among firms in the propensity to patent any given technological advance because only a very small percentage of the total are of high monetary value and the large number of patents are of low value. The large number of patents of low value introduces considerable uncertainty into the firm's decision to patent, as noted earlier.[54]

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[51] M.E. Mogee, "The Process of Technological Innovation in Industry,: A State-of-Knowledge Review for the U.S. Congress," in Special Study on Economic Change, v. 3 Research and Innovation. U.S. Congress. Joint Economic Committee print. 96th Congress, 2d session. (Washington, D.C.: U.S. Govt. Print. Off., 1980): pp. 171 - 256.

[52] M.E. Mogee, Technology and Trade: Some Indicators of the State of U.S. Industrial Innovation. U.S. Congress. House of Representatives. Committee on Ways and Means. Subcommittee on Trade. Committee print. 96th Congress, 2d session. (Washington, D.C.: U.S. Govt. Print. Off., 1980).

[53] B. Sanders, J. Rossman, and L. J. Harris, "The Nonuse of Patented Inventions," The Patent, Trademark, and Copyright Journal of Research and Education, v. 2 (March 1958): 1 - 60.

Nor do the time lags between R&D and patents, and between patents and commercial introduction seem to behave in the expected fashion. Pakes and Griliches did not find significant lagged relationships between R&D and patents.[55] Basberg found that patenting related to an important innovation did not precede full-scale development but reached its maximum at the time of first commercial exploitation.[56] Other studies have shown that patents vary in the time taken to commercialize them[57] and that major innovations vary significantly in the time from conception to commercialization.[58]

Pavitt argued that both R&D expenditures and patent statistics are indicators of "innovative activity."[59] It seems to me that the correct approach would be to assume that patent statistics indicate "innovative activity" and to determine, through further analysis and correlation, whether patent statistics are more strongly related to or affected by "technological" factors such as R&D and breakthroughs or by "economic" factors such as business cycles and foreign trade relationships. The tentative conclusion of some work of this type is that patent statistics are a fairly neutral indicator of science and technology output.[60]

How adequate are patent statistics as an indicator of innovative activity? Patent statistics have been criticized on the grounds that patent counts do not reflect differences in the quality of inventions. Some patents cover breakthrough inventions that form the basis for whole new industries, while others cover minor improvements and even trivial inventions. Thus at the level of individual patents, one patent may clearly not equal another. This is not a problem unique to patent statistics, however; it is common to data sets where quality is an important factor -- e.g. R&D expenditures and scientific and engineering personnel. When large numbers of patents are involved, analytical techniques can possibly be developed to correct or adjust for differences in the value of patents and the roles they play in different industries.[61] Scherer's work on interindustry differences in the propensity to patent, the work of Schankerman and Pakes using patent renewal rates to measure private economic value, and the work on bibliometric techniques to measure patent quality are steps in the right direction.[62]

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[54] Pavitt, "Patent Statistics," p. 8.

[55] Pakes and Griliches, op. cit. The two seemed to move almost simultaneously, although that could be due to a large "noise component" in short-term changes in patenting within firms.

[56] Basberg, op. cit.

[57] Sanders, et al., op. cit.

[58] These studies are summarized in Mogee, "The Process of Technological Innovation," pp. 248 - 251.

[59] Pavitt, "Patent Statistics," p. 9.

[60] L. Soete and S. Wyatt, "The Use of Foreign Patenting as an Internationally Comparable Science and Technology Output Indicator," Scientometrics, v. 5 (1983) 31 - 54.

Patents do not reflect all innovative activity because not all technologies are patentable. For example, computer software is increasingly being copyrighted because of questions about its patentability. In some countries chemical compounds per se are not patentable. Nor are all patentable inventions patented, because an inventor or firm may decide that the costs of patenting are higher than the benefits or may decide to use some alternative form of protection such as trade secrets. The number of patents issued is affected by many factors in addition to the number of inventions made such as patent laws, patent office procedures and resources, economic outlook, and the perceived value of patent protection. If these influences are systematic with respect to the analysis being conducted, the results may be misleading. This will be discussed in the next section, on sources of systematic bias.

2. Sources of Bias in Patent Data

The second major criticism of the use of patent statistics as indicators of innovative activity is that there are significant biases in the data. Pavitt discussed the following sources of bias:

1. Differences among countries in economic benefits and costs associated with patent protection

2. Differences among technologies and sectors in the relative importance of patents as protection against imitation

3. Differences among firms in the propensity to patent the output of innovative activity.[63]

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[61] W. S. Comanor and F. M. Scherer, "Patent Statistics as a Measure of Technical Change," Journal of Political Economy, v. 77 (1969): 393.

[62] Scherer, "Propensity to Patent;" Schankerman and Pakes, op. cit.; Narin, Carpenter, and Woolf, op. cit.

[63] Pavitt, "Patent Statistics, p. 82."

As Christian reminds us, to be a good indicator of a phenomenon, a statistic must be regularly and consistently related to that phenomenon.[64] It is not clear that patents are regularly and consistently related to innovation because a variety of factors that are independent of innovative activity affect the number of patents. For example while most patents are obtained to protect innovations, in some industries significant numbers of patents are obtained for the purpose of licensing or for blocking competitors. In such industries the number of patents issued does not represent innovative output alone but also these other incentives. The relationship of patents to innovation also seems to vary by whether the technology is product- or process-related, process inventions tending more often to be kept secret. The propensity to patent inventions varies by industry and technology, tending to be high for technologies like chemicals and pharmaceuticals where inventions are expensive to develop and easy to copy. The propensity to patent also appears to be lower in industries in which there is a high level of government support for R&D.[65]

Differences in national patent laws have also been pointed to as a source of bias in patent statistics.[66] While national laws differ, it has been said that they are internationally harmonized perhaps more than any other type of law.[67] Bosworth found that differences in national patent laws, to the extent he was able to quantify them, were not a significant factor in explaining international differences in the number of U.S. patent applications abroad.[68] The method of comparing shares of patenting in a common foreign country avoids this problem.

There have also been changes in the relationship of patents to innovation over time. Studies at the level of countries and of firms have found that the propensity to patent, as measured by the ratio of patents to R&D expenditures or number of scientists and engineers has declined over the last twenty or so years.[69] It is not clear whether this represents declining productivity of R&D over this period, i.e., fewer inventions, the decision to patent a smaller proportion of inventions, a greater proportion of inventions that are not patentable, or increases in the quality of patents obtained. There is some anecdotal evidence that companies are patenting a smaller share of their inventions. If so, this could distort interpretation of patent trends unless some estimate of the effect can be made.

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[64] J. Christian, Patent Data as an Indicator of Invention: The Firm's Propensity to Patent. Workshop on Patent and Innovation Statistics. (Paris: Organization for Economic Cooperation and Development, May 1982), p. 4.

[65] See for example, Scherer, "Propensity to Patent;" Griliches, ed., op. cit.; and C. Taylor and A. Silberston, The Economic Impact of the Patent System (Cambridge University Press, 1973).

[66] Soete, "Technological Gap Trade Theory," p. 641.

[67] D. L. Bosworth, "The Transfer of U.S. Technology Abroad," Research Policy, v. 9 (October 1980): 380.

[68] Ibid., pp. 378-388.

[69] Griliches, op. cit.; Evenson, op. cit.

It has also been shown that the relationship of patents to innovation changes over the life cycle of major technologies and the industries based on them. Walsh's study of major innovations in the chemical industry found that the major breakthroughs were accompanied by a small peak in patenting activity, but that a much larger peak in patenting occurred later, accompanying a large number of secondary, or improving, innovations.[70] Thus a given level of patenting in the early stages of the new technology was not equivalent to that same level at a later period but rather represented much greater technological strides. Walsh argued that patent data must be used in conjunction with qualitative knowledge of the history of the technology to allow correct interpretation of patenting peaks.

3. Problems of Classification

A problem specific to the use of patents to measure innovative activity by industry is the organization of the data. The U.S. Patent Classification system is designed to help patent examiners search the prior art; therefore it is based upon the technological features of inventions that make them patentable. The basis for classification is largely functional and therefore the classes and subclasses do not always correspond closely to industries or technologies as commonly defined.

Currently, the primary method of drawing a correspondence between patent statistics and industries is through the use of the OTAF Concordance between the U.S. Patent Classification system and the Standard Industrial Classification (SIC) system.[71] The basic problem with a concordance approach, which is based on patent subclasses, is the lack of a good match between patent subclasses and industry categories. Even individual patents sometimes do not fit neatly into industrial categories. For example, patents on brakes often do not specify whether the brake is for an automobile or an airplane; in some cases it could in principle be used in either. Using the OTAF Concordance, Pavitt found statistically significant relationships across ten OECD nations (excluding the United States) between national shares of industrial R&D and U.S. patenting by sector. In some sectors such as aerospace, other transportation, and metals and metal products, the relationship was less close, perhaps reflecting concordance problems.[72] The assignment of SIC categories to individual patents may represent an improvement. The Canadian PATDAT data base, which covers Canadian patents, does this.

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[70] Walsh, op. cit.

[71] The OTAF Concordance was the subject of a recent workshop and report. Office of Technology Assessment and Forecast, Review and Assessment of the OTAF Concordance Between the U.S. Patent Classification and the Standard Industrial Classification Systems: Final Report (Washington, D.C., January 1985).

[72] Pavitt, "R&D, Patenting, and Innovative Activities."

V. CONCLUSION

Despite considerable questioning of the adequacy of patent statistics, a great deal of effort has gone into using patents as indicators of innovative activity. This has occurred because of the inherent potential of the data for analysis and the increasing need for quantitative, objective measures of innovative activity. In the course of this "experimental" period of about 15 years, information has been gained about the sources and magnitudes of bias and misinterpretation, the circumstances in which patent counts are a valid indicator and those which they are not, and the kinds of data that are useful complements to patent statistics.

All of the questions and criticisms have not been answered by any means. Patent data would benefit from more sophisticated statistical treatment to devise methods of overcoming such shortcomings as variable patent "values" and time lags. Nonetheless substantial progress has been made. Patent data can be satisfactory technology indicators. As a recent major project on "R&D, Patents, and Productivity," concluded, "to a first approximation, one can use patent data as indicators of technological activity in parallel with or in lieu of R&D data."[73]

What does all this have to do with technology forecasting? The adequacy of patent statistics to measure and describe technology in the past and present is the necessary basis for using them to forecast technology. The name and history of OTAF are instructive in this regard. Although OTAF had "forecasting" in its name, it made only a couple of attempts at forecasting. The reason it did not do more was that the technology "assessment" (read indicators) part of its mission -- i.e. experimenting wih patent analyses, getting the word out, and making the data available -- consumed the available resources. Similarly I would argue that little progress has been made to date on forecasting with patent statistics because the fundamental questions about the validity of patent statistics as technology indicators had not yet been adequately resolved. Most of the analytical effort was aimed, appropriately, at answering those fundamental questions. This helps to explain, I believe, the articles and reports that speak of forecasting in their titles but in fact use patents to describe past and current trends in technical activity.

1. Prognosis

The usefulness of patent statistics as indicators of innovative activity argues that more effort should be put into using them in forecasting and planning. A variety of types of forecasts would appear possible. They can be divided into three basic types:

1. Patent statistics based on counts of a limited number of standard data items, i.e. bibliographic data, routinely captured for all patents on electronic data bases, e.g., the OTAF data base.

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[73] Z. Griliches, "Introduction," in Griliches, ed., op. cit., p. 14.

2. Patent statistics based on a limited number of standard data items extracted manually from the text of selected patents -- e.g. the approach of Battelle Pacific Northwest Laboratories.

3. Analysis of the technical information in individual patents to determine the objective of the invention, the previous state-of-the-art, the nature of the solution, etc.--e.g., the approach of Martino or of Hack and Matejka.

There are also hybrids of these methods, such as the approach of Mlodzik in using family member counts to identify important patents and then studying the chemical structures or other technical information in those patents.

Forecasts of the first type would basically be projections of the level of patent activity, its national origin, and its ownership. Such projections could be made for various technologies, firms, industries, and countries and for purposes of comparisons. They can be used in a number of ways.

Firms may want to compare their areas of innovative activity to those of competitors, to know if they should step up effort in a certain area of technology. Or they may want to know what new processes or products competitors are working on. They may wish to evaluate a firm's technological portfolio when considering a merger or acquisition. They may want to identify potential sources of technology for licensing-in or potential licensees for licensing-out. Analysis of international patterns of patenting may reveal something of international business strategy of competitors. Researchers and investors may use patent statistics to identify firms that are particularly active in rapidly moving fields of technology.

Governments may use these kinds of patent statistics to evaluate the health of national science and technology systems and to make comparisons with other countries. Such information may contribute to decisions on levels and allocation of research funding for example. Patent statistics can also be useful in analysis of trade competition issues by providing information on the developing technological position of certain industries in other countries and the international business strategies of firms.

The second type of forecast, which is based on statistical analysis of "bibliographic" data, citations, and other data items in patents, provides a variety of decision or planning tools. In addition to projections of patent activity, which the first method can provide, it provides more qualitative information on the field of technology being studied -- such as its stage of development and patterns of proprietary technology that are being staked out. A firm may use this method to determine if a field of technology in which it operates is growing, mature, or in obsolescence. If the field is mature or in obsolescence, it may wish to get out of the field and enter new, emerging fields of technology to maintain growth. A pattern of heavy patenting around a basic patent by a firm may indicate a major thrust in that area. This method is somewhat labor-intensive because it requires manual extraction of data items from the text of the patent. Further refinements of the variables examined, however, and improvements in information technology may make this method easier to use.

The third type of forecast results in detailed projections of future technical designs and functions. This comes closest to "technology forecasting" as commonly defined. It relies on technical analysis of the information contained in patents, rather than on statistical analysis of the bibliographic data or citation data, as do the first two types. To be most useful, it requires technological expertise to interpret and analyze the information, although the use of key words may reduce this requirement. This type of analysis might alert a firm to work being carried on elsewhere that could result in a breakthrough threatening its product line. It may help corporate planners learn earlier what types of technologies will be available to deal with particular problems in five to ten years.

Patents have been a relatively underused source of data for forecasting and planning, partly because of the difficulty of access and data handling and partly because of questions about their adequacy as indicators of innovative activity. Certain problems remain, such as the termination of the OTAF program in the Patent and Trademark Office and the expense of extracting information that is not included on electronic data bases. Also, patent data users must become familiar with the data and its idiosyncrasies, but these are becoming better documented (see Pavitt's review, for example). Nonetheless, the problems of working with patent data have been significantly reduced and the idea that they can be used as indicators of innovative activity has been established as fundamentally legitimate. Therefore one would expect to see increasing use of patent data in a variety of forecasting and planning applications.

DISCUSSION

Mr. FUQUA. Thank you very much, Dr. Mogee.

Are there any changes in the patent law and the regulations that you would recommend that would promote or encourage more basic research and probably more productivity out of basic research?

Dr. MOGEE. I am not really an expert on patent policy so much, but I would say that it seems that the University-Small Business Patent Amendments of 1980 were a step in the right direction, by giving universities the right to take title in the patents that were developed in their facilities using Government funding. And it would allow them to take more interest in commercializing those patents and, to the extent that the patents bring in revenues, the revenues would presumably be channeled back into basic research.

Mr. FUQUA. Are there any foreign patent practices that would be useful to us in this country that you are aware of?

Dr. MOGEE. With respect to basic research, no, I can't say that I know of any.

Mr. FUQUA. What about laws such as patent and antitrust that act to reduce the effectiveness of the Government's investment in basic research? Do you have any suggestions in that area?

Dr. MOGEE. I guess the thrust of my comments fits somewhat with the presentation that was given just before by Rustum Roy and Deb Shapley. I believe that there is a lot that can be done to improve the effectiveness of Federal funding of basic research by strengthening the connections and the transfer from research into industrial applications.

I guess that would be my position too.

Mr. FUQUA. That would include not trying to insist on the recoupment if it had been federally-supported research, of recoupment of Federal funds that may have been involved in that as an encouragement to commercialize the patent?

Dr. MOGEE. That is right, I think that would be a mistake.

Mr. FUQUA. To have recoupment?

Dr. MOGEE. To have recoupment.

Mr. FUQUA. Are there any other changes in current patent policy that you see? I know we have modified that in recent years to exclude not-for-profit and small business.

Dr. MOGEE. And the universities are included under the not-for-profit.

Mr. FUQUA. Yes.

Dr. MOGEE. Beyond that I am not knowledgeable enough to speculate on what would be good changes in the patent law.

Mr. FUQUA. Mr. Packard.

Mr. PACKARD. Thank you, Mr. Chairman.

The patent problems internationally, Dr. Mogee, are creating problems for—at least our trade issues—many countries do not recognize U.S. patents.

Do you see any progress being made, or efforts being made, to bring about some kind of an international policy on protection of the information and protection of products?

Dr. MOGEE. I understand—I am not involved in them personally—that there is quite a bit of activity going on primarily on a bilateral basis, between the United States and other countries that are perceived as being trouble spots in terms of intellectual property protection. And that is being done in a number of ways and through a number of forums, and at a number of levels of negotiations.

I would expect to see some progress on that.

Mr. **Packard**. You feel that it is needed?

Dr. **Mogee**. My understanding is that in certain countries that offer inadequate intellectual property protection, U.S. products are being copied and pirated and reexported. So that I think it makes sense to try to obtain more standardized intellectual property protection in the countries around the world.

Mr. **Packard**. In view of that, do we need to change our own patent laws to accommodate those kinds of international objectives?

Dr. **Mogee**. This is a very highly technical field of international intellectual law and one that I am not really expert in.

My understanding is that there are international treaties, such as the Paris Convention, to set certain standards for intellectual property. But the basic intellectual property laws are national laws administered by the nations.

I think the U.S. laws are in conformance with whatever international conventions there are at this time.

Mr. **Packard**. Let me ask the question a little bit differently then: Do you think that there should be some changes in patent law or patent regulations in this country, not from an international perspective, but to promote its own basic research as well as making more productivity out of that basic research?

Dr. **Mogee**. My remarks won't pertain so much to basic research because that is not really the main thrust of the patent law either. But there is a law—a bill—under consideration, I think, that would protect the import of products that are produced overseas with processes that are patented in the United States. Most countries have this kind of protection but the United States does not currently. That seems to me to be a reasonable thing to do.

Mr. **Packard**. The numbers on your charts, I notice that you do have numbers associated with institutions as well as industrial firms. Do you associate the numbers with success or expertise in these areas of research?

Dr. **Mogee**. I think patent data can be used to say for an industrial firm—to look at the numbers and compare it to another firm, let's say in the same industry, you might find the areas where they are putting out the most R&D effort, and you might infer from that they are likely to have a strong proprietary interest in that certain area of technology.

I don't know if you would use it to say that one of them is more innovative than the other, but it would be a more qualitative description of the direction that their R&D is going.

Mr. **Packard**. Your statistics certainly do then bear out what we have already known and that is that Japan is outstripping the United States considerably in these areas.

Dr. **Mogee**. You are looking at table 2?

Mr. **Packard**. You have 4 of the the top 10 in the United States that are Japanese firms.

Dr. **Mogee**. Right.

Mr. **Packard**. Now, I would have to assume that if you had a chart of Japanese firms and their numbers, you wouldn't find a single American company that would qualify among their top 10.

Dr. **Mogee**. I don't know about that.

Mr. **Packard**. Well, I don't know either. That is only an assumption. I would have to assume that we have no American companies in Japan that can compete with Sony and some of the other major Japanese companies in terms of numbers of patents within their country. But it does certainly point up the fact they are very aggressive——

Dr. **Mogee**. Right.

Mr. **Packard** [continuing]. In turning basic research and basic information into a marketable product, and they are outstripping us over and over again on it, both here and abroad.

Dr. **Mogee**. I think one thing that this shows us is that some of the Japanese corporations have targeted the United States market for their high technology products, and that they are coming over here and they are patenting because they want to sell their products here. I think those figures really reflect the market invasion.

Mr. **Packard**. Thank you, Mr. Chairman, I have no further questions.

Mr. **Fuqua**. Thank you very much, Dr. Mogee, we appreciate you being here this morning.

[Questions and answers for the record follow:]

SCIENCE POLICY TASK FORCE

Hearings on Research Funding as an Investment

May 1, 1986

Followup Questions for Dr. Mogee

1. **When will a "Patents and Technology Forecasting" (Footnote No. 1) be available to the Task Force?**

   A more detailed discussion of the subject appears in "Patents and Innovation Forecasting," a paper presented at the 1985 Annual Meeting of the American Association for the Advancement of Science, which was previously submitted to the Task Force for inclusion in the record.

2. **On page 2 of your prepared testimony you allude to studies conducted in the United States, Great Britian and Australia which addressed "questions dealing with the output and input of basic research." Later on page 7, III, you note that you are "not aware of any results from these efforts to date." Does this remark reference the previously cited studies? If not, please identify these studies and detail the nature of the analyses and conclusions derived therefrom insofar as they may bear on investment in basic research.**

   To the best of my knowledge the studies in Great Britian, Australia, and New Zealand have not been published yet.

3. **A point was advanced by another witness that the patent principle should be extended to include the results or findings of basic research. Do you view this as a feasible means to bring credit to an investigator and provide him with an avenue for realizing a return for his labors?**

   The question of whether some type of incentive patterned after the patent system that exists for inventions should be applied to the findings of basic research is a complex one that deserves further analysis and thought. In my opinion, which is preliminary and not based on a thorough analysis, it is probably not a good idea for a couple of reasons.

   The current system of science, technology, and innovation depends importantly on the free availability of basic research findings to all. Scientists conducting basic research are motivated primarily by the desire to increase knowledge and disseminate it broadly. If economic gain becomes more important to basic research scientists than the love of knowledge, they may become more secretive, because in order to obtain an economic return from the research it must remain proprietary. There are already reports of this phenomenon in biotechnology and other areas of basic science that have near-term commercial potential. The effect of increased secrecy in science would likely be to slow down the overall rate of scientific progress, since less new knowledge would be entering the public domain and hence would not be stimulating the thoughts and research of other scientists. Given these considerations, incentives that stress public recognition may be more appropriate than economic incentives for basic researchers.

Another consideration is the relationship between basic scientific research and commercial success, and the implications that has for the administrative feasibility of such a system. The idea of granting a patent-type right for a basic research finding assumes that the direction of causality runs from basic research to technological innovation. There is much evidence, however, that the chain of events often runs in the opposite direction. Moreover, in many areas technological innovation progresses largely independent of research, drawing instead on prior technology and engineering.

The classic example of the invention of the steam engine preceding the discovery of the laws of thermodynamics can be used to illustrate the problem posed for a patent-type system. Suppose a "patent" has been granted to Maxwell for his discovery of the laws of thermodynamics. Presumably then all commercial products and processes utilizing that law would be required to pay royalties to the researcher; this is clearly unrealistic since practically all machines and processes utilize the law of thermodynamics. This example also clarifies why the current patent law excludes "laws of nature" from patentability.

Even when basic scientific findings preceed technological progress, the path is often long and tortuous. Much basic research may never have commercial application. Or if it does, it is generally in combination with other scientific and engineering findings. Thus, it would be very difficult to establish, at the time it was made, that a scientific finding had commercial value. It would also be difficult to trace back the scientific antecedents of a commercial innovation, at the time it was made. Attempts to do this -- e.g. Hindsight, TRACES, and The Sources of Invention -- reveal that: (1) successful innovations generally result from multiple inputs of scientific, engineering, and inventive efforts, often from multiple people, organizations, and countries; (2) it is often difficult to obtain agreement on the true sources of these ideas, the true inventors, the nature of linkages, etc.; and (3) such traces are expensive. All of these considerations would impede the efficient and equitable administration of a patent-type incentive system for basic research.

Mr. **Fuqua**. Our cleanup hitter this morning will be Dr. Richard P. Hallion of the History Office of the 6510 Air Base Group at Edwards Air Force Base.

STATEMENT OF DR. RICHARD P. HALLION, HISTORY OFFICE, 6510 AIR BASE GROUP, FLIGHT TEST CENTER, EDWARDS AIR FORCE BASE, CA

Dr. **Hallion**. Good morning, Mr. Chairman.

Mr. **Fuqua**. Good morning, or afternoon.

Dr. **Hallion**. Indeed.

It has been a long session this morning but I think an extremely profitable one. I found it personally fascinating. This issue is a quite serious issue, I think, that is facing the Nation.

I had a prepared statement which I would like to have entered into the record.

Mr. **Fuqua**. Yes; we will make that part of the record if you wish to summarize.

Dr. **Hallion**. Fine. I will summarize it and then add some other thoughts spurred by the discussion this morning.

First of all, to clarify again a statement I made in my prepared statement, I am speaking here to you today as a private citizen, as a historian of technology and not in fact as a representative of the Department of Defense or the U.S. Air Force.

I think after hearing this morning's testimony, my feeling is that what we need in this Nation is a rediscovery of what we might term the Baconian impulse. If we take a look at the work of Francis Bacon, one of the catalytic figures in the Scientific Revolution, we find that here was a man who creatively blended both the impulse of science and the impulse of what we have come to term technology. He saw science as manifesting itself in works.

I was particularly stimulated by Professor Roy's refreshing testimony that, really, if we take a look at technology in a broad view, we can see technology as a producing force behind science itself. Certainly today, if we take a look, for example, at the instrumentation by which we measure and review, analyze, and produce science, that instrumentation is a product of the technological impulse.

If we look around this room, we see many examples of the creative blending of science and technology, ranging from planetary exploration through remote sensing of the Earth through the exploitation of active technologies today.

Now, I feel that to a degree I am also here offering a synthesis view, if you will, between some of the positions taken this morning by Professor Roy and, if you will, the other team—those unidentified advocates of things like supercolliders. Frankly, I think that if we take a look at the status of American technology, we see that we face extreme challenges—particularly in the electronics area.

But we also find that we have some very great strengths. I think particularly within the aeronautics field—we find that we have a national base of technical excellence in the area of aerodynamics, structures, propulsion, and flight controls technology that at this point puts us in a leading competitive position worldwide. It, of course, offers us great strength in our national defense posture.

And I think it exemplifies the beneficial relationship that we have had through the years of research by the National Advisory Committee for Aeronautics and its successor, the National Aeronautics and Space Administration.

If I was to analyze those two in terms of what made them strong—what brought us to this point we are in today—I would say that they always exhibited a very clear view of near-term needs and long-term needs, and we are shrewd enough not to pursue extremely long-term needs that would result in the solution to problems for the day after tomorrow, but not necessarily the problems of tomorrow.

We can contrast that approach, if you will, with the approach taken by Nazi Germany's science and technology during the Second World War. Here was an evil state, of course, that subjugated and subordinated its science to extremely misguided purposes. And the Nazi state really destroyed its science and technological capabilities in the name of ideology.

But beyond that, where they did pursue technology, we find very often that the technology they pursued was inappropriate for the task at hand—something we can all be very thankful for. They were not pursuing the kinds of technology that could have resulted in a battlefield victory in the Second World War.

The Allied Nations, typified by the research programs of the United States and Great Britain, on the other hand, had very excellent management of the science and technology process. I think we got a good account of this in a book by an eminent British scientist, R.V. Jones, called The Wizard War, and out of this came an Allied triumph by the time of VE Day.

It is very interesting, then, if we take a look at the postwar course of science in this country, starting with one of the works, "Science: The Endless Frontier" by Vannevar Bush. We find that if we examine, for example, Vannevar Bush's own work, he was an individual who was an extremely distinguished scientist but, unfortunately, rather shortsighted on what could be accomplished in technology. He can be contrasted, I think, with scientists who were running overtly technological organizations. I am thinking of two in particular: Hugh Dryden of the Bureau of Standards, and later the National Advisory Committee for Aeronautics, and Theodore von Kármán. Both of these individuals were classicly trained physicists. In the case of von Kármán, you have a man who was an emigre from Weimar Germany. He could see the writing on the wall. These individuals had profound impact upon the establishment of the aeronautics and scientific community dealing with aeronautics in the United States.

If we were to take a look at certain crisis points, or crisis times, if we are faced within the evolution of aeronautics in this country, I think a very good example where we saw a creative blending of Federal support and private support could be found in the 1920's, specifically the activities, et cetera, around a fund—a private philanthropic fund—called the Daniel Guggenheim Fund for the Promotion of Aeronautics. Here was a fund established by an entrepreneur. Its total monetary value was approximately $3 million, and it established, basically, aeronautical engineering education as we know it within the United States.

At the same time, it provided a great stimulus to commercial aeronautical development by creating a model airline between San Francisco and Los Angeles, and by undertaking actual research and development work in such fields as short takeoff and landing design, and in the field of aircraft instrumentation for so-called blind flight.

It did this through a process of peer review. It also did this in close collaboration with the Bureau of Standards, the National Advisory Committee for Aeronautics, and the Army Air Service in those days, and the U.S. Navy. Through this process we saw a unified and extremely influential effort that resulted by 1940 in most engineering graduates working with major aircraft firms in this country being graduates of the Guggenheim school system.

The National Advisory Committee for Aeronautics—the predecessor of today's NASA—was born amid the progressive era's interest in economy and efficiency in Government. But very wisely through the years of the NACA, from 1915 through 1958, those individuals running the Government did not seek to see a measure of NACA productivity strictly in economic terms alone.

I think, for example, if we take a look at the development of air transport aircraft in the years from the First World War through the 1930's, roughly the years from crude experimentation up to the era of the DC-3—which was the first aircraft capable of making a profit simply by carrying passengers—we would see a situation that had people tried to justify the development of those rudimentary air transport aircraft on the basis of some economic payoff, I think we would have stymied ourselves. We would not have in point of fact pursued the development of those vehicles, because the economic payoff we were expecting at that time was quite nebulous.

This brings up a point that I think afflicts us in the measurement of technology and to that degree the measurement of science itself. It is relatively inconvenient for decisionmakers in these fields to try to assess the merits or demerits of a science field or a technology field in terms of the internal technique or knowledge level within those fields.

For example, Congressmen face a number of challenges. They face a number of problems in decisionmaking, and as is pointed out in the committee's agenda report, the time where you can expect to have a political figure—paraphrasing Thomas Jefferson, who is also thoroughly knowledgeable in science and technology—well, those days are really past. You have to rely, I think, upon staff to do that.

Very often in the field that I deal with—the field of history of technology—I find that many of my historical colleagues analyze technological growth in terms of the search for some sort of economic payoff—reduced production costs, economic benefit, seat cost per mile, things like that—in assessing, for example, aeronautical development. But these are not always the best measures.

The best measures are the search for efficiency within the field. For example, within the field of aerodynamics, the development of aircraft having increasingly high lift to drag ratios, or reduced power requirements, or enhanced takeoff and cruise performance. Those kinds of measurements are the kinds of things that can best be undertaken by a group of peers, by the practitioners themselves within the field.

If we take a look at the NACA and NASA, we find that those organizations were rooted in a crisis. We had invented the airplane in this country, and by the time of the First World War already we had been surpassed by European aeronautical technology. The lack of a Federal laboratory system dealing with aeronautics—something patterned on the Bureau of Standards model or the model of the Advisory Committee for Aeronautics in Great Britain caused congressional leaders, some military figures, and some academicians to argue for the creation of a national advisory committee that would also be aligned and joined with a laboratory tradition.

And out of this came the National Advisory Committee for Aeronautics.

By the 1930's, so great had the contributions of the NACA been to the aeronautics field, that European designers were complaining that for the best design data—handbook data, if you will—they had to rely not on the work of their own national engineers and institutions, but rather, upon the work of the National Advisory Committee for Aeronautics.

If we take a look at the years since the Second World War, particularly with regard to NACA and NASA research, we find that the two organizations responded very well to the twin challenges that were affecting aeronautics at midcentury—the challenge of the so-called turbojet revolution—this massive switch from the era of the propeller-driven airplane to the era of the jet, and then the challenge of the supersonic airplane.

At the time, neither of those concepts could have been justified on the basis of economic benefit.

I think this brings up another point, and that is, when we take a look at high technology and significant science, very often we find that it is applied first in areas where economic benefit is at best nebulous. It is usually applied within the arena of national defense, because without national defense, you really don't have anything else.

And then there is a traditional tech transfer out of the national defense community into the civilian sector. We have seen this, for example, with gas turbine technology; we have seen this with high speed aerodynamics. For example, going from the era of the straight wing airplane to the swept wing airplane. The first aircraft that had swept wings for high speed flight were military fighters and bombers. Transports began to receive those items in gas turbine propulsion then in the 1950's.

At the same time, from a personal standpoint, I would caution against the notion of tying defense needs to an anticipated civilian sector payoff. That is somewhat akin, I think, to the notion of commonality in aircraft acquisition. We have an argument that comes up constantly: Why can't the Navy and the Air Force get together and build the same kind of airplane for the same kind of mission? You could save all these development costs in airframes.

If you take a look at it, the differing requirements of the services and the differing needs mitigate against that. And if you in similar fashion, if you try to tie defense research and development to the needs of civilian research and development too closely, I think you will really hamstring both of those.

In closing, I would like to make a couple of comments generally concerning the problem of assessment. I have addressed already what I see as weaknesses in trying to seek strictly economic benefit assessment out of technological development.

I think that rather than looking too closely at assessment, perhaps we should be looking in another direction. We should be taking a look at the development and exploitation of forecasting methodologies. Futures forecasting is sometimes considered a black art. But I think that it can play a key role in assessing the level of technology and science that we have now and where we are going from it.

A recent example that I think has accomplished a great deal is Air Force Systems Command's Project Forecast II, patterned on an earlier effort of the 1960's that reshaped future aerospace development at that time. This is the kind of oversight that to me is meaningful—assessing the nature and state of a particular branch of science and technology and then letting its practitioners do their job.

I fear that attempts at accountability and research that look for quick economic or social benefit, quick transfer from the lab to the commercial sector, and then rejecting those efforts that seem serendipitous or simply off the mainstream might result in an oversight of quite another kind: and that is the oversight of enforced stagnation of the research process that, for example, nearly stifled turbojet research in Great Britain before the Second World War, and which to a large degree was responsible for the destruction of the British aerospace industry after 1945.

A good example here is given by the postwar so-called X-series research aircraft program. These were experimental vehicles, many were built. Some were not extremely successful; they did not influence subsequent aeronautical design. But they were vehicles that were developed to answer questions. And even if we received a negative answer out of that, that alone was sufficient to justify their development. That is the nature of flight research using experimental aircraft. The fact that a configuration did not necessarily influence the subsequent development of aeronautics, did not mean that the program was wasted—quite the opposite. Information was learned, false paths were revealed; and such it is, I think, with research in general.

The quest is what is important. If researchers feel that they must constantly justify their work in terms of success stories, that fear alone can lead to scientific and technological paralysis as researchers limit their work to what is evolutionary and attainable rather than what is revolutionary and desirable.

Thank you very much.

[The prepared statement of Dr. Hallion follows:]

STATEMENT OF DR. RICHARD P. HALLION BEFORE THE TASK FORCE ON SCIENCE POLICY OF THE HOUSE COMMITTEE ON SCIENCE AND TECHNOLOGY, MAY 1, 1986

It is both a pleasure and an honor to have been invited to testify before this Task Force on the issue of research funding as an investment. As a professional historian of aerospace technology, I have always found this aspect of research to be a fascinating one. It is in that capacity that I speak before you today, and thus my views and opinions expressed are those of a private citizen, and should not be construed as representing official positions of the United States Air Force, the Department of Defense, or any other governmental agency or body.

I would like to begin my testimony with a passing reference to the writings of Francis Bacon, one of the great catalytic figures of the Scientific Revolution, who stated that the goal of science is works. In contrast to a basic underpinning of previous scientific thought--namely the notion of thinking of science only in terms of the search for knowledge (a notion dating to the Greek philosopher Plato), Bacon thus advocated what we would recognize as a technological payoff for scientific inquiry.

There has always been a dualism in the goals and methods of science and technology; one historian has referred to them as "Mirror-image twins," having common methodologies but their own distinct goals. Some even argue that in the process of scientific and technological research, significant differences exist to the extent that these fields should not really be considered even mirror-image in nature. To the scientist, typified by the physicist, the acquisition of new knowledge is oftentimes in and of itself the justification for seeking such knowledge. Questions of a practical payoff are not usually of significance; with such research often conducted in an academic environment, the critical question is typically one of acquiring continuing funding support, usually via grants. To the technologist, typified by the engineer, the search for scientific knowledge is justified by the quest for new capabilities generated and new technology developed. The technologist seeks a practical payoff, and since much technology is pursued in an industrial environment, that payoff is often allied with a concomitant drive for profitability. Nowadays, with some emerging areas, we are seeing a blurring of these traditional differences; genetic research offers one such example.

Throughout the course of American history, there have been mutually beneficial relationships between the scientific and technological communities, the various branches and departments of the Federal government, the academic world, and the commercial sector. Some of these have been alluded to in the Task Force's Agenda for a Study of Government Science Policy. Certainly a cursory historical review of the aerospace field offers numerous examples of symbiotic assistance.

The history of the National Aeronautics and Space Administration and its predecessor, the National Advisory Committee for Aeronautics, can furnish a useful case study for the question "Does it pay for the government to put money into scientific and technological research?" Created in 1915 by an act of Congress, the National Advisory Committee for Aeronautics was an institution that typified the Progressive Era's concern over economy and efficiency in government. Commonly known as the NACA, the agency had as its mandate "to supervise the scientific study of the problems of flight with a view to their practical solution." One of the major reasons for its creation had been recognition by leading scientific and engineering figures such as Smithsonian Institution Secretary Charles Walcott, Naval Constructor Holden C. Richardson, and various military officials and Congressmen that the United States (which had invented the airplane in 1903) already occupied a position of relative inferiority to the European nations because it lacked a suitable national aeronautical laboratory. Taking a cue from Parliament, Congress modeled the NACA upon Great Britain's Advisory Committee for Aeronautics, but went beyond this by empowering the Committee to establish a laboratory which eventually emerged as the NACA Langley Memorial Aeronautical Laboratory, the predecessor of the NASA Langley Research Center today. By the 1930's, so dominant had this organization become that technical staff members of Great Britain's National Physical Laboratory were quoted in the British aeronautical press as stating, with some exasperation, that British aircraft design firms preferred to base their design work upon the results of the NACA's research, rather than that of indigenous British institutions. What exactly had the NACA accomplished?

Primarily its technical staff members used Federal research monies to investigate basic problems in aerodynamics, structures, and propulsion: the major fields of aeronautical inquiry. For example, the NACA established a whole family of wing cross-section shapes called airfoils, enabling aircraft designers to construct their aircraft with wing shapes offering greater lift and less drag. Additionally, the agency used its funding to undertake development of improved research tools and facilities, particularly wind tunnels. The NACA built its research around the tunnel, and its contributions to tunnel development, including the variable-density tunnel, the full-scale tunnel, the slotted-throat transonic flow tunnel, and supersonic and hypervelocity tunnel facilities were truly remarkable. The agency complemented its tunnel work with aggressive flight research and testing, and development and testing of specialized ground facilities. Out of this work came tools and techniques to undertake basic aeronautical research. The agency's scientists and engineers undertook this basic research themselves, and out of this came a data base that resulted in the high-performance all-metal military and civilian monoplanes of the 1930's.

By the end of the 1930's, then, the pattern for future NACA aeronautical research had been established--a partnership of ground research, and research aloft, using the sky as a laboratory. As there was a partnership in the research methodologies, the NACA benefitted from a cross-fertilization with other agencies, industry, and academic institutions. Each year, agency officials would meet with governmental, airline, and industry officials in conferences to assess the state of aeronautical research, to show the aeronautical community what the agency was doing, and to receive feedback as to its work and the implications of that work for production military and civilian aircraft. This was the mutually supportive climate that led by the middle 1930's to a situation where NACA research dominated not only American aeronautics, but foreign aeronautics as well. As a result of this, by and large American aircraft were superior to their foreign competitors. American transport aircraft came to dominate the world air transport market. The United States developed efficient long-range military bombers to a degree unmatched by other nations, including our Allies of the Second World War. Finally, as just one example of the application of NACA research, the North American P-51 Mustang, the finest all-around fighter of the Second World War, utilized a NACA low-drag wing section, and benefitted from intensive wind tunnel studies that helped turn its basic design from merely promising to genuinely outstanding.

The two major revolutions affecting aeronautics at mid-century were the turbojet revolution and the supersonic breakthrough. Though the NACA (as with other federal, industrial, and academic bodies) largely missed the significance of the gas turbine engine, it was a key player in undertaking basic aerodynamic research aimed at developing the first supersonic aircraft--airplanes capable of flying faster than the speed of sound. At the beginning of the 1930's, agency engineers had studied the requirements for supersonic flight. In 1942, the NACA began research studies that eventually spawned the interagency X-series research aircraft program beginning with the Bell XS-1, which became the first aircraft to fly faster than sound in 1947. NACA research in the 1940's gave the then-radical sweptwing and delta wing shapes to American aeronautics, and did so (contrary to popular belief) without influence from German wartime aeronautical research. NACA research in the 1940's and 1950's opened up the supersonic arena for aircraft designers by providing a clear understanding of transonic and supersonic aerodynamic flow conditions around aircraft, as well as by furnishing a thorough analysis of the behavior of aircraft at those speeds, typified by the agency's work on the transonic and supersonic area-rule principle, the development of the all-moving horizontal tail for high-speed flight control, the agency's analysis of the so-called inertial coupling problem (commonly termed roll divergence), and the development of the outboard wing pivot for variable wing-sweeping aircraft designs.

The NACA was an agency whose leaders interpreted its mandate as directing the study of flight within or at the fringes of the atmosphere. Thus, it did not devote attention to spaceflight *per se*, though selected NACA scientists and engineers were very interested in that field, particularly in the complex problems of flying out into space with some sort of a vehicle generating aerodynamic lift, retaining control over such a craft both in the atmosphere and in space, and then returning it through the atmosphere to land safely on the earth or in the ocean. Research at the Ames and Langley centers in the 1950's furnished aerospace science with such concepts and tools as the blunt body reentry principle, hypersonic delta boost-gliders, hypersonic tunnels and shock tubes, and conceptual studies for genuine hypersonic craft such as the North American X-15, which emerged as the nation's major research aircraft program of the 1960's, undertaken jointly by the

NACA (later NASA), the U.S. Air Force, and the U.S. Navy. Nevertheless, of course, when the nation's executive and legislative leadership decided to restructure aerospace research within the federal government in the wake of the Sputnik episode, that leadership wisely chose to create a new organization built in large measure around the NACA, but with a mandate for space research as well. Though it is not my intent to discuss the space program in depth, I feel it appropriate to note that the agency responded decisively under the leadership of such individuals as James Webb and Thomas Paine to President John F. Kennedy's call for a lunar landing before the end of the decade of the 1960's. The Eagle at Tranquillity Base while the Columbia orbited the moon was an American triumph in general and a NASA triumph in particular.

NASA faced a number of difficulties in the 1960's because of having to devote most of its attention to space. As a result, the aeronautics side of the agency often found itself locked into competition for resources and funding with the space side of the house. Despite this situation, the agency's aeronautical research in the 1960's and into the 1970's continued the long-standing NACA tradition of excellence and influence. Good examples here are the agency's work on design configurations for supersonic and hypersonic cruise aircraft. Though this nation made a conscious decision in 1971 not to pursue a commercial supersonic future, the design studies that NASA pursued in the 1960's are, for the most part, as relevant today as they were then. Certainly the contemporary interest in the Aerospace Plane program mirrors a research effort whose origins involved several agencies' work, including NASA's, in the 1960's. During the 1960's and 1970's, agency scientists and engineers first applied digital electronic flight controls to testbed aircraft, demonstrating an experimental flight control technology concept that has since gone into service, and which is transforming aircraft performance and design. The agency developed the so-called supercritical transonic wing design concept, validated it in flight research, and refined it so that it, too, is now a standard production item on new generation transports. Likewise, the agency began exploring abandoning the traditional metal aircraft structure for a non-traditional composite material structure. Composite structures likewise are beginning to revolutionize the design process. Aircraft design has always involved the integration of diverse and complex technologies. NASA work in these fields, as with the work of other federal bodies such as the Department of Defense, is leading to new generations of aircraft featuring composite structures, advanced propulsion concepts, digital electronic flight controls technology, and radical relaxed stability design principles that promise to transform aerospace vehicle design and capabilities as radically as the shift from the fabric-covered biplane to the all-metal monoplane, or the shift from the propeller-driven airplane to the jet. To return to the question asked earlier in this statement, "Does it pay for the government to put money into scientific and technological research?", I think that the question, with respect to the NACA and NASA experience, must be a resounding "Yes, absolutely."

I have used the NACA-NASA as a case study because of my familiarity with the history of these two agencies, particularly with respect to aeronautical research. It is possible that examination of other organizations, both federal and private, could lead to a different answer to the question above, though I seriously doubt it. During the early part of this century, private organizations (typified by philanthropic funds) and academic institutions could often, on their own, make remarkable accomplishments in the science and technology fields. For example, during the 1920's, a private fund created by entrepreneur Daniel Guggenheim virtually established aeronautical engineering education as we know it today in this country, developed blind-flying instrumentation that resulted in a quantum leap in flight safety, explored the requirements of so-called STOL (for short takeoff and landing) aircraft, and even established an experimental airline using newly designed airliners and ground facilities that influenced the subsequent development of the national air transport and airways network. It did all this over four years, and at a cost of less than $3 million dollars, the total amount spent over the entire life of this fund. But those days, following the Second World War, are gone forever. While I strongly believe that we must encourage the private researcher and developer, we must expect that the major advances to come in the science and technology fields will come from the institutions we already have, or the ones we will establish. These institutions will have continuing and critical requirements for federal funding because nowadays, science and technology are costly and lengthy; the days of simple science and simple technology are past. In sum, researchers today must have the inquisitive and adventurous spirit of the pioneers that have gone before, but should not be expected to work under the conditions that their predecessors did.

As the Task Force wisely noted in its previously cited Agenda, "The days when a prominent politician such as Thomas Jefferson was also thoroughly familiar with science are long gone." The process of critical analysis is a complex one; political leaders have many responsibilities and cannot be reasonably expected to comprehend the intricacies of scientific and technological issues. Likewise, (again as the Task Force noted), administrators in science and technological fields often are too distanced from contemporary science and technology frontiers to be fully capable of assessing the nature of work their organizations do. Artificial measures of merit, such as economic benefit, are difficult to apply, and, I think, may generate misleading results. For example, in my own field (the history of technology), many scholars assess technological evolution in terms of some form of economic benefit--for example, reduced production costs. They often do this, I fear, because of a basic inability to assess the merits or demerits of a technology on its own terms. Air transport operations began in the years prior to the First World War, but it was not until the era of the DC-3 that an airline could make money simply by hauling passengers without need to rely upon air mail subsidies. It was necessary to live with nearly a quarter century of inefficient transports to arrive at the technological point where the packages of improvements developed over the intervening years could be integrated into a fully successful and indeed revolutionary airliner. I personally think that seeking financial accountability for aeronautical research and development during that time period might well have stifled the necessary on-going process that eventually generated the DC-3.

I would suggest that greater attention might be paid to futures forecasting methodologies, illustrated by such schemes as the so-called Delphi interrogation system. Our comprehension of the future and what our requirements are is totally dependent upon how well we comprehend our present, and that, in turn, is dependent upon how well we comprehend from where we have come. Recently, Air Force Systems Command embarked on Project Forecast II, patterned on the Forecast I study of the 1960's that reshaped future aerospace development. Forecast II resulted in many promising ideas, and was intended, from the outset, to seek revolutionary, not evolutionary, approaches. In my view, this is precisely the kind of forecasting activity that should be encouraged across the board in scientific and technological institutions, if for no other reason that we as a nation no longer have the grace and time to play scientific and technological catch-up if we are caught napping by foreign competitors.

This is the kind of oversight that is meaningful: assessing the nature and state of a particular branch of science or technology, and then letting its practitioners do their job. I fear that attempts at accountability in research looking for quick economic or social benefit, quick transfer from the lab to the commercial sector, and rejecting those efforts that seem serendipitous or simply "off the mainstream" might result in an oversight of quite another kind: the oversight of missed opportunities and the enforced stagnation of the research process that (for example) nearly stifled turbojet research in Great Britain before the Second World War and which (to a large degree) shattered that nation's excellent aircraft industry in the years after the war. We must remember, I think, the very nature of research: if one could guarantee a priori the results of a hypothesis, we would not require research. An example can be seen from the postwar X-series research aircraft. Here were radical, highly experimental vehicles acquiring information on new aerodynamic, structural, and propulsion frontiers. Some were intended to examine certain design configurations, to assess whether or not such configurations were useful. Many proved worthwhile and influenced subsequent aircraft design. Others did not and were rejected. But tnat is the nature of flight research using experimental aircraft. The fact that a particular configuration did not prove worthwhile when actually developed did not mean that the program was wasted--quite the opposite. Information was learned, a false path was exposed. Research involves proving concepts and making new discoveries. Sometimes promising concepts prove illusory and new discoveries fail to appear. But the quest is what is important. I fear that if researchers feel they must constantly justify their work in terms of success stories, that fear alone will lead to scientific and technological paralysis as researchers limit their vision to what is evolutionary and attainable rather than what might be revolutionary and desirable.

I began this statement by alluding to Francis Bacon, a pioneer of the experimental method. I would like to close it by quoting from his seminal work Novum Organum, for I feel it is of particular relevance to us as we ponder the future of American science. He stated "By far the greatest obstacle to the progress of science and to the undertaking of new tasks and provinces therein, is found in this--that men despair and think things impossible." Let us work so that future historians will never have the chance to pronounce that judgment upon our society.

Again, I wish to thank the Task Force and particularly its distinguished chairman, for the opportunity to appear. I will be happy to elaborate upon this statement and answer any other questions you may have.

Mr. **Fuqua**. Thank you. Dr. Hallion, I have some questions, unfortunately, we have another vote, and I am going to have to leave. But with your permission, I will submit them to you in writing and maybe you can respond to them.

Dr. **Hallion**. I will be very happy to.

Mr. **Fuqua**. I apologize for the inconvenience, but when the House is in session, we do have votes.

Dr. **Hallion**. That is quite all right.

Mr. **Fuqua**. Thank you very much.

Dr. **Hallion**. Thank you very much.

[Whereupon, at 12:35 p.m., the task force adjourned to recovene on Tuesday, May 6, 1986 at 10 a.m.]

[Questions and answers for Dr. Hallion follows:]

SCIENCE POLICY TASK FORCE

FOLLOW-UP QUESTIONS AND ANSWERS FOR DR HALLION

Q1. The Bush Report of 1945, while supporting a stronger role for the Federal Government in supporting basic and applied research, also cautioned that too strong a government role could reduce the private sector's interest in supporting research. In your view, is the balance today between Federal and private research support about right, and what should that balance be in future decades?

A1. I believe that the balance between Federal and private research is about right. In part, there is an inevitability about this that cannot, in my view, be changed: scientific and technological research is so expensive and complex today that private sector organizations simply lack the resources on their own to pursue it to any degree greater than they are now doing. Much of their research is, in fact, supported by grants and other Federal programs to such an extent that we really have three national categories of research: Federal research conducted by government employees in governmental facilities, Federally-supported research undertaken in private institutions by individuals on government contract, and private research undertaken by private individuals and institutions using private funding resources. Determining a balance in the future would not be easy and might well, in fact, be relatively meaningless. To a degree, the "balance" is determined by external forces; availability of research facilities for research work, availability of money (either private or Federal) for such research, the size and scope of the work effort required to support such research, and the research interests of the organizations (private and Federal) themselves. A balance could be mandated, but these factors would always work to frustrate such an idealized structuring of the private vs. Federal research relationship.

Q2. Should Congress broaden its present view of research funding, which essentially favors basic research, to include more emphasis on applied research as well and thereby bridge the gap between scientific knowledge and technology? What should industry's role be in conducting applied research? Is this the missing link that has enabled foreign competitors, notably Japan, to take a commanding lead in many technological markets? Is American industry remiss in not being more aggressive in this domain?

A2. Congress has already shown a keen and appropriate interest in the nature and direction of research funding in America. It would be quite proper for Congress to expand this interest to the area of applied research. I personally feel that the relationship between science on one hand and technology on the other is so complex that it is difficult to state with any degree of certainty that this would result (as the question states) in "bridg(ing) the gap between scientific knowledge and technology." Technological development often leads directly into scientific discovery, and there is often a transfer from scientific knowledge into applied scientific research--both cases that run against the popular perception of "science" triggering "technology." Industry's role is quite properly to pursue applied research with a view to generating marketable science and marketable technology. However, I do not believe that somehow applied research is a "missing link" that foreign competitors (notably Japan) have exploited over their American counterparts. American industry, particularly in the electronics and aerospace fields, has been quite aggressive and successful in applied research efforts--witness, for example, continued American dominance in the fields of super-computer development, advanced aerodynamics, advanced composite structures, and digital flight control systems. What is possibly a missing link is the state of education in the United States. Now, more than any previous time in our history, we need literate, perceptive, and scientifically and technologically aware teachers capable of firing the minds of students so that those students are able to make informed choices as to career fields, and are able to compete successfully with their foreign peers. Unfortunately, we have a situation instead where the teaching profession itself is afflicted by serious morale problems, and faces a "quality control" problem that all too often produces marginal and indeed mediocre classroom instruction. The long-term implications of letting this situation continue are more than disturbing--they are downright frightening, since they ultimately bring into question our ability to survive as a vibrate, viable nation.

Q3. Knowing that the policy of the Federal Government is to support basic research, do you feel that private industry has taken a "let George do it" approach and not responsibly supported an appropriate share of the cost?

A3. I think that by and large, private industry has supported as much basic research as it is able to do so. For the reasons stated in answer (1) above, I feel that private industry is usually constrained by funding and facilities questions (as well as the pressures of having to compete on the open market with rivals, which further limits the amount of time, funding, and effort that can be put into research) from undertaking as much basic research as private entrepreneurs would like to support. Certainly, private industry finds itself in a more conducive position with regard to applied research as opposed to basic research. The private record on applied research (as stated in answer 2 above) is generally a good one.

Q4. Would greater federal support to industry for basic research be more effective than direct participation by the government in such research?

A4. There is always the danger in supporting purely industry-sponsored research that the industrial organization will prioritize and establish research goals on the basis of its own short-term profit-oriented (i.e.: market-oriented) needs and desires. The Federal government can afford to undertake longer-range research because it does not have to deal with the same market pressures that afflict the private sector. Because its research horizon is longer and broader, it can chart new territory in basic research before there is a need to generate practical benefit from it. Likewise, because of its sense of detachment from the needs of industry, the Federal government is in an ideal position to undertake responsible research oversight and coordination. The history of the Axis powers in the Second World War, especially Nazi Germany, offers important lessons for research managers and planners. The Nazi state tended to rely upon industry for basic and applied research. While the latter might well have been desirable, the former certainly was not. When this policy was coupled with the regime's ideologicization of the scientific and technological process in the name of Nazi dogma, the result--for which we can all be thankful--was inefficient chaos. Because of the research failures of the Nazi regime, Nazi Germany was doomed as early as 1941. It just took until 1945 for the beast to stop twitching. Today, we see a de facto situation where the government is largely responsible for basic research and the private sector is responsible for applied research. While it would not be desirable to institutionalize and formalize this process (because science and technology are dynamic, and such an action would stultify and constrain the development of both), it is well that we recognize that this relationship does, in fact, exist. A useful approach might be for Federal scientific and technological agencies to seek closer relationships with private organizations doing applied research work, particularly in the role of oversight and coordination, and possibly via direct funding support for selected applied research efforts as well. This latter point is, of course, quite controversial and care would have to be taken to avoid both actual and apparent conflict of interest and favoritism. Nevertheless, it is an option that should be studied further.

Q5. Is governmental support of basic research and industrial support of applied research an effective way to manage science policy?

A5. As stated in answer (4) above, a de facto situation already exists whereby the Federal government supports basic research and industry supports applied research. In the time-critical and funding-limited environment of the 1980's, this situation seems both inevitable and acceptable. In the best of all possible worlds, both the private and Federal sectors would be able to support both basic and applied research as they saw fit. But this situation simply does not accomodate the realities of the decision-making environment affecting American science and technology. As explained more fully in the above answers, the present situation is, in my view, not only the reality we have to live with, but an acceptable and generally satisfactory situation as well. What is required, I think, is: a) greater emphasis on our national education policy to generate the scientists, technologists, and informed citizens we will need in the 1990's and beyond; b) improved coordination by Federal scientific and technological agencies over their own basic and applied research and the basic and applied research of private organizations; and c) exploration of new methods whereby the Federal government can coordinate and support selected applied research by the private sector, when that research can be shown to have national as opposed to merely proprietary value.

APPENDIX 1

ADDITIONAL STATEMENT FOR THE RECORD

STATEMENT FOR THE RECORD

to

House Science & Technology Committee

Science Policy Task Force

Hearing

on

Research Funding As An Investment

"Estuarine Research As An Investment"

by

Dr. Frank Perkins

Director, Virginia Institute of Marine Science
Dean, College of William & Mary

on behalf of the Marine Division

National Association of State Universities and Land-Grant Colleges

Washington, D.C.

1986

ESTUARINE RESEARCH FUNDING AS AN INVESTMENT

The Marine Division of NASULGC represents almost all of the marine academic community. The Association is the oldest higher education association in the country, and celebrates its 100th year in 1987. We are delighted to be able to participate in the series of hearings on science policy.

The latest United States census reveals that 65 percent of the population lives within 100 miles of our nation's coasts. That figure is expected to jump to 75 percent by the turn-of-the-century. The stresses being imposed upon the coastal and estuarine environment include every aspect we have learned to equate with increased population - from land use controversies to water quality to quality of life. The economic potentials involved in land use decisions involving coastal development are staggering. The average acre of farmland in the U.S. is valued at $695; the most productive land at $2,000. On the coasts, a comparable land value is $35,000 to $125,000 per acre.

Society's needs and desires compel managers and legislators to make difficult choices. Fortunately, every recent survey indicates that most Americans are unwilling to damage or destroy the overall environment in exchange for economic gain. However, at the same time the people expect economic vitality and growth in their own communities. It is therefore imperative that planners, managers and legislators have accurate, comprehensive information in order to guide economic development.

Prior investment in estuarine and coastal research has been providing positive societal return, and any reductions in this investment will be detrimental to the momentum gained. As societies' demands and needs increase, so does the need to provide this basic support.

In recent years, research in estuarine systems have provided vital information to allow the development of predictive tools to forecast the consequences of various loadings on the systems and the costs of various management strategies. For example, there have been major advancements in predictive models to forecast water quality characteristics. While still imperfect, such models are now routinely used in waste load allocations. Current basic research activities show very strong promise for providing the data necessary to improve our ability to predict water quality parameters. As the predictive models are improved, managers will be able to make less conservative decisions and reduce costs.

Advances are being made in formulation of transport models for estuarine sediments and associated toxic chemicals. With additional research in sediment transport processes, these models will provide managers the ability to predict the dispersion rates and patterns of toxic chemicals.

Ecosystem models are also under development. These tools provide insights on whether, and how, an impact at one level of the food chain propagates to other levels of the chain of living marine resources. Such models are the logical extension of water quality management which addresses maintenance of standards thought to be suitable for marine life.

One of the triggers for environmental concern is the decline of various commercial fin fish species. The cause of the decline may be due to natural response, climate variability, or any one of several anthropomorphic changes. Clearly it is essential that the cause be determined so that managers do not invest scarce funds in managing advancements in predictive models which are unable to discern how much of annual stock variation is due to seasonal wind fields and water temperature, or other things.

Another benefit of modeling for the future may well be the "zoning" of an estuarine system as is done on land. Planners can then balance the intensity of development by multiple users, allowing different user groups to co-exist with maximum benefits for all.

Recent estuarine research in aquaculture is providing the potential to improve yields of valuable seafood products by intensifying production in certain locations while allowing other types of development in other areas. Those research efforts in the field of aquaculture are now beginning to pay handsome dividends for both fishermen and businessmen in the Chesapeake Bay. Hard clam hatchery and grow-out operations developed at the Virginia Insititute of Marine Science and in use on the Eastern Shore of Virginia are in an advanced stage of development and are now economically viable. In one operation, using the most advanced application of research-developed technology, 5-8 million clams are out of the hatchery and have been planted in growing areas. The 1986 anticipated harvest in this one operation is 500,000 to 1,000,000 clams with expectations of an annual harvest of 5 times this number. There are several other operations that will approach and soon collectively exceed this volume. Current wholesale little neck clam prices range between 12-16 cents each and it is realistic to assume that the current cost of production ranges between 6-8 cents each. These favorable price differentials have been made possible only as a result of biological research on the hard clam.

The recent rapid expansion of the soft blue crab industry, where landings have doubled in the Chesapeake Bay over the past three years, can also be directly attributed to the application of knowledge gained from scientific inquiry dating back to the early 1970's. There are many examples of expansion and construction of new shedding facilities as a result of this contribution of 125,000 dozen per year of soft shell crabs valued in excess of $1 million. Similar examples can be demonsrated in all states producing soft crabs. Added value attributed to this transition from hard crab to soft crab can be appreciated only when one considers that a hard crab is worth 5-8 cents, a pre-molt crab is worth 25-30 cents and a soft shell blue crab is worth 75 cents-$1.00. Another application of knowledge gained from the same research activities has led to the development of a soft shell rock crab industry which now utilizes that previously discarded resource in the lower Chesapeake Bay.

Around the United States, research in aquaculture has affected production and marketing of shrimp, catfish, oysters, fresh water trout and salmon. The increased interest on the part of the public in improved nutrition through seafood consumption makes this area of research not only economically valuable, but particulary popular.

Increased consumption of fish has been linked with reduced heart disease, and this is especially important in view of the $72 billion spent last year on heart and lung problems in the U.S.

The public clearly has a strong desire for more interaction with the coastal environment. Increasing coastal land values are in part attributable to two of the largest growth industries in the United States - tourism and recreation. Recent figures reveal that the beach is the number one vacation choice for our citizens, with one-half of our population taking vacations on the coast. Among the other coastal recreation activities, boating alone is a $12 billion per year business. Tourism and recreation industries also lower unemployment by employing the largest numbers of young and relatively unskilled labor.

While the whole coastal area is attractive to visitors, it is the sandy beaches that remain the major tourist attraction; however, each year billions of dollars must be spent to protect, maintain and rejuvenate our nation's public and private beaches. Coastal erosion causes tremendous personal and commercial property loss each year. In this area, too, estuarine research is leading to solutions.

A short-term beach response model recently developed at the Virginia Institute of Marine Science allows scientists to assess readily the stability of any given beach in terms of the likelihood that the beach will erode, accrete, or remain unchanged. In future applications, this model and its expected refinements should significantly enhance the ability of estuarine and coastal planners and managers to anticipate shoreline changes and to adopt the most appropriate and least risky development strategy. Also, long-term models currently under investigation will require adequate funding to bring about longer term solutions.

Clearly understanding the complex estuarine and coastal systems will lead to more efficient and cost-effective development as well as offering protection for the environment. However, there is another important field of estuarine research which is offering mankind the opportunity to better understand his own biological systems. Through studies of the estuarine environment and its organisms, bio-medical researchers are learning more about the mechanisms of human disease. Simple aquatic organisms often provide fundamental clues to the more complex human systems. In addition, we have learned that estuarine organisms in the egg and larval stages respond with extreme sensitivity to environmental chemical changes which may not effect juvenile and adult states until toxicity reaches much higher levels. Studies using estuarine organisms in the early stages of development allow researchers to develop chemical model systems which will give managers the ability to monitor and manage water quality with much greater effectiveness. Generally, biotechnology offers tremendous business potential, and the use of biological models will be in great part responsible for this improved return.

These are only a few examples of the increasing contributions of marine science to management and use of our coastal and estuarine systems. From basic research which provides the fundamental understanding of the systems and their inhabitants, to applied technology that increases productivity of a fishery, research is allowing us to protect, develop and also to enhance our coastal environment.

In the coastal and estuarine world, we are increasingly aware that while we struggle with the issues of the day, we must also invest our time and talents in the future. Decisions concerning matters such as ocean waste disposal, fishery management, coastal development and channel dredging will all directly affect the health and prosperity of the coastal and estuarine environment. As we continue to respond on a day-to-day basis to the problems and issues, the efforts of the estuarine and coastal science communities must be adequately supported to permit acquisition of the fundamental knowledge, which will enable us to answer the questions of tomorrow - the social, economic and environmental health of our future depend on it.

APPENDIX 2

ADDITIONAL MATERIALS FOR THE RECORD

THERMOSCIENCES DIVISION
STANFORD UNIVERSITY
MECHANICAL ENGINEERING DEPARTMENT
STANFORD, CALIFORNIA
94305

May 14, 1986

RECEIVED
MAY 26 1986
COMMITTEE ON SCIENCE
AND TECHNOLOGY

The Honorable Ron Packard
U. S. House of Representatives
Room 316 Cannon House Office Building
Washington D.C. 20515

Dear Congressman Packard:

This letter is an additional comment on the recent hearings concerning Science as an Investment. An attached short Vita provides my background in this area. I ask that you enter these comments in the record.

As the hearings indicated, unless science is coupled to downstream development, production and marketing, it has no economic value. Several questions then arise, "What is the nature of this coupling ?" "How can the coupling be made effective?" and "What role should the Federal Government play, if any, in making sure that the science is done and the coupling occurs?"

Historically, we have all tended to view the coupling of science to economics as serial and linear. Our long-held belief is: "The science is done, and this initiates the chain that leads to commercial impact." The chain of events set in quotes does sometimes occur, but is in fact the rare case, not the common one. It is only part of the story. Two ideas must be added if we are to understand commercial innovation properly and thus take appropriate actions.

First, much of the research of greatest economic impact is stimulated by the problems made evident by the work of development and the experiences in use of both the products and processes. To be effective and efficient, commercial innovation cannot be treated as a serial process beginning with science. It is, and must be treated as, an interacting matrix with many feedback loops. These facts are set down in a model developed for the purpose which is attached in the form of Stanford University Mechanical Engineering Department Report INN-1B (marked Appendix III). Places of publication and typical reactions of R & D leaders to this model are shown in the report.

Second, not all science has the same potential value in economics. Some forms of science are critical to economic success in various market sectors. Other forms of science are essentially irrelevant to economics. Moreover, the correlation between those parts of science that are new, "cutting-edge" topics and commercial success is weak. Some old areas of science are commercially vital; some new "cutting-edge" areas are irrelevant. Since scientists nearly always place top priority on the cutting edge problems in their own particular field (as they should), this evidence by itself cannot be used in formulation of federal policy without risk of misalignment between areas of support in science and the needs of the economic system. Some areas of science thus need to be viewed from the position of government policy as worthy of support only as "cultural items"--like the arts. There are other areas of science that we allow to languish, only at the peril of the well-being of our economic system. I provide some examples of each type of science in Appendix II. Please be aware, however, that these lists are incomplete and are therefore not yet a basis for action. Formulation of a complete list, appropriate for setting priorities, needs a national study as noted below.

If we accept and face the facts of the two preceding paragraphs, what do they suggest for the role of government in fostering and supporting science and technology? Is it sufficient as policy to support science on all fronts and leave further developments to private industry? Will such a policy keep the United States competitive economically in a wide range of industries? The answer is suggested in the testimony to the Committee, particularly that of Lamont Eltinge, and of Rustum Roy and Deborah Shapley in the excellent program organized by Mel Ross. Roy and Shapley indicate the problems that have been occurring in the current system and why they occur. Eltinge provides a particularly well thought out list of what is needed for success and where the critical factors lie.

A particularly important point was made by Eltinge. He emphasized that the critical factor is the knowledge in the heads of the R & D people. Knowledge on the shelf, in the literature, known by scientists in isolated labs but not known to the designers of the systems is irrelevant. It is the knowledge already known, or acquired in a timely manner, by the designers of the products and processes (including both technical and market knowledge) that is central to the results and plays a critical role in determining economic success (or failure) of companies.

The two preceding paragraphs lead us to the following question, "How can we insure that exceptionally able technical people are properly prepared and distributed throughout industry?" This is largely accomplished by providing education that prepares and interests very able workers in the necessary areas. Once recruited into a given area of work, technical people tend to stay in that area for long times--for a decade or more, and often for a full working career. Moreover, a decade or so of work and education beyond a B.S. education is essential for growing real experts in any technical area--old or new. Because of our support for cutting edge science in the universities since World War II, (and not much else), we have interested students in new areas and largely abandoned the older areas as topics of research and innovation. However, many of the older areas still remain vital to our economic well-being as Seireg and Leontev noted in the recent hearings. The example of corrosion Leontev cited is but one of a number shown in Appendix I. Seireg and Leontev and others also pointed to engineering research centers strongly coupled to industry as a modality for closing these gaps in our system. Another important aspect of such centers is their ability to transcend narrow disciplinary boundaries and integrate the totality of knowledge required. The need for many disciplines will more and more characterize the important advances.

Thus far federal government has set up three such engineering research centers. However, to close the gaps, we need several centers (to foster healthy competition) in each area of important economic concern. In each critical area, we need two or three centers to foster healthy competition. This implies a total number of centers something like 100-200 rather than the three now funded or the dozen or so planned. No one now knows the number of such engineering research centers that are needed or the specific areas they need to cover. Those facts need to be established by a careful Delphi-type study at the national level. Such a study could be conducted by the IRI, by the National Academies of Engineering and Sciences (with the Engineering Academy taking the lead) or jointly by these institutions. Data coupling innovation goals to input/output analysis (from Leontev and Seireg) would aid such a study.

This kind of overall planning and setting of priorites can only be done at the national level. Overall planning and goal-setting is not the same thing as interference in the details of the work or mandating specific technological developments. The Congress with the Executive Branch set the priority of landing a man on the moon. The details were then delegated to an appropriate institution, and the work funded. Industry on its own would not have proposed or carried out this goal. The goal once set spawned not only hardware development, but also much fundamental research. Our current economic needs for innovation create a similar challenge. The question is whether we will meet that challenge, or will take the comfortable decision that all is well, and we need not change the course of our science policy set by Conant and Bush forty years ago.

Are such engineering centers feasible and what is their cost? We have enough experience with such centers (established at a few places through individual efforts) to understand how they need to operate to be successful. However, a consortium of workers with varied backgrounds is needed to assess the areas where they are needed and to set priorities. Members of such a consortium could be identified by NAE and/or IRI if requested to do so.

A few million dollars per year is sufficient to create and support an engineering center in an appropriate university or other laboratory. Such a center, if functioning properly, will attract additional support from industry thereby leveraging the expenditures and insuring coupling. For thinking purposes, let us take a high figure, say four million dollars per year, as an average for each center. Let us also take a high figure for the number of centers needed, say 200. The total cost of such centers in full operation would then be 800 million dollars per year. This is the same amount that is now intended as operational support for the new super-collider. However, the engineering centers have a much smaller start-up cost. Which of these alternatives, many engineering centers or one super-collider, is more likely to benefit the economic system?

The super collider will beyond much doubt produce many Nobel prizes. On the other hand, the probability of the super collider providing economic benefits, even in the long run, is small. So much energy is needed to create effects at the sub nuclear levels of concern, that useful commercial applications are unlikely. Consequently, the oft-cited analogy to nuclear processes is probably misleading.

Without at least many engineering centers and the improved coupling they would bring between new ideas and industrial practice and the seeding source they would supply to insure proper distribution of our limited supply of people of high technical talent, our economic system is unlikely to be able to meet the challenges now confronting us from many other nations in the increasing internationalization of markets.

This comparison does not answer all the questions or serve all the needs. It does however point to the difference between two policies: one that supports science in the terms defined by internalities of science, and one that plans for the needs of the economic system, sets priorities and assures not only the support of science but also the back-and-forth coupling of science into the downstream development, production and marketing system that lies at the heart of economic success.

Sincerely,

SKline

S. J. Kline
Prof. Mechanical Engineering
Prof. Values, Technology, Science and Society
Member National Academy of Engineering

Encl: Appendix I - List of areas in innovation,
Appendix II - Vita,
Appendix III - Report INN-1B

cc: George Brown, Don Fuqua, Melvin Ross (House Committee on Science and Technology, Washington, DC)

APPENDIX I (Letter to the Honorable R. Packard, 15 May 1986)

A. Areas where increased support of generic research and coupling of research through innovation to production is likely to have important economic benefits over long time spans--an incomplete preliminary list. *

1. Machine tools
2. Process control (theory and equipment)
3. Turbomachinery (pumps, turbines, compressors etc)
4. Electric Power Equipment (as distinct from electronic)
5. Ceramics, Polymers, Composite materials--glass...
6. Metalurgical processes (ferrous and non-ferrous)
7. Joining processes (welding, brazing, stapling,...)
8. Textile machinery, machines for sewing clothes, leather...
9. Postal Machinery
10. Forming processes (casting, forging, die-casting, ...)
11. Corrosion
12. Lubrication
13. Metal fatigue and fracture
14. Computerized data taking
15. Machinability, high speed tools

B. A list of research areas that are unlikely to generate important economic returns--they may warrant support for other reasons, for example: military objectives, health requirements...*

1. Astronomy
2. Chemical Warfare agents
3. Deep Sea Studies
4. Very high energy (subnuclear) physics
5. High Altitude Atmospheric Research
6. Space Exploration
7. Deep earth core studies

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* These lists are illustrative and incomplete. A thorough national study with input from all sectors of industry, from research groups and from economists, is needed to formulate such lists as a basis for policy formulation.

May 1986

APPENDIX II

ABBREVIATED VITA STEPHEN J. KLINE

CONSULTING ON TECHNICAL PROBLEMS:
General Electric (6 Divisions); General Motors Research Labs; Procter and Gamble; E. I. duPont de Nemours; Creare Inc.; Brown Boveri et Cie.; United Technologies...

CONSULTING ON INNOVATION PROCESSES:
General Motors Research Labs; Procter and Gamble; Catalytica; E. I. du Pont de Nemours; NTI;

TEACHING: Prepared and offer course on Innovation, Stanford Univ.

Fifteen years work on fundamental concepts concerning the relations of technology and society as part of the Values, Technology Science and Society Program at Stanford. Member National Committee for Science through Science, Technology and Society now preparing materials for high school science classes.

INDUSTRY-UNIVERSITY RELATIONS
Founder (in 1958) and Director (15 years) of Thermosciences Affiliates Program at Stanford. Roughly 20 corporate members. Program includes means for active exchange of information and personnel between research and engineering design groups.

PUBLICATIONS: Created improved model of innovation processes in industrial societies; published in three journals with differing emphasis; publication with extended remarks in 4th Journal pending. See remarks about this model in Appendix III.

AFFILIATIONS, HONORS
Fellow ASME, Member National Academy of Engineering;
Numerous honors for Technical papers in three countries.

EDUCATION
B.A., M.S - Stanford University, ScD - M.I.T., all in Mechanical Engineering. NSF Senior Postdoctoral Fellow Harvard, Mathematics.

RESEARCH, INVENTION, INNOVATION, AND PRODUCTION: MODELS AND REALITY

by

Stephen J. Kline

Report INN-1B

Thermosciences Division
Department of Mechanical Engineering
Stanford University
Stanford, California

November 1985

May 1986

INFORMATION ABOUT REPORT INN-1B FOR READERS

Open Literature Publications

This report, Department of Mechanical Engineering, Stanford University, Report INN-1B has been published in the following places in abbreviated forms with differing emphasis: differing emphasis:

1. "Research Is Not a Linear Process," Research Management, XXVIII, July-August 1985.

2. "An Appropriate Model for Industrial Innovation," Science, Technology and Society Newsletter, No. 49, Sept. 1985, Curriculum Newsletter of STS Program at Lehigh University, Bethlehem, PA 18015. ISSN: 0275-8075.

3. "An Overview of Innovation," (with Nathan Rosenberg of Stanford University Economics Dept.), in *The Positive Sum Strategy: Harnessing Technology for Economic Growth*, National Academy of Sciences Press, 1986. ISBN 0-309-03630-5.

Publication in TECHNOLOGY REVIEW is pending. This version, with Nathan Rosenberg, covers remarks on the relations to economics and suggests implications for research managers.

COMMENTS FROM READERS THUS FAR -- MAY 1986

It may be useful for the readers to know the general nature of comments and the sources from which they come.

Thus far about thirty written and oral comments have been received from individuals. The model has been presented at seminars in four industrial companies and in three forums at Stanford. In all these instances no basic exception to the ideas and paths set down in the model have come forth.

Some useful ideas for improvements have been offered. One of these owing to Dr. Harold Hall of Xerox concerned the classification and name for the Design activity; this suggestion has been incorporated in this 1B version of the report. Prof. Richard Rosenbloom of Harvard Business School has also suggested it would be clearer to show the feedback links from the market to earlier phases of activity by a box along the bottom comparable to those for knowledge and research on the top. This will be done in the next reprinting.

Specific efforts have been made to obtain comment from individuals with long successful experience in managing R & D. These comments are typically of three general kinds: (i) "We find the model conforms with our experience;" (ii) The model draws together ideas we had developed over the years but previously had no way to organize." (iii) "It helps us think more clearly and plan better for innovation." No negative comments have been received from this group. Only one individual in this group suggested his company already knew what the model sets down; he is in a large company built from the start by successful innovation. A leader in military innovation suggests the model is much the same in military work except that threat to military safety and preparedness replaces the commercial market. He also finds the model clarifies his thinking on the topic.

RESEARCH, INVENTION, INNOVATION, AND PRODUCTION: MODELS AND REALITY

by

Stephen J. Kline

Report INN-1B

Thermosciences Division
Department of Mechanical Engineering
Stanford University
Stanford, California 94305

February 1985
(Revised November 1985)

SJK:R-15
INN-1B
Rev. Nov. 1985

RESEARCH, INVENTION, INNOVATION, AND PRODUCTION: MODELS AND REALITY

by S. J. Kline[*]

ABSTRACT

Some scientists have suggested that research is the central factor in industrial innovation. However, many studies show that the immediate precursor of the majority of industrial innovations is prior technology.

Some economists have suggested that "market pull" or "human needs" is the major source of industrial innovations. However, a careful analysis by Mowery and Rosenberg shows this suggestion is at best too simple and is usually much overstated.

Both suggestions seem to arise from IMPLICIT use of an oversimple "linear", single-pathway model of innovation processes. However, no clearer model has been generally available. This paper suggests an improved model, called the "linked-chain" model. The linked-chain model is based on the author's observations of the structure of innovation processes as seen from three decades of consulting experience in a variety of industries.[†]

In the linked-chain model, five pathways for innovation exist, and all five are important, as demonstrated by historical examples. The linked-chain model also highlights certain misconceptions and difficulties in current USA R&D practice and thereby raises significant questions for R&D policy.

ACKNOWLEDGMENTS

In order to check the concepts, an earlier version of this paper was sent to approximately 60 able engineers and researchers with long experience in innovation. Nearly half of them responded, providing assurance that their experience was consistent with the ideas presented. Nearly all these replies contained constructive comments. Length requirements prevent individual acknowledgment, but most comments have been incorporated and are much appreciated.

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[*] Professor of Mechanical Engineering and of Values, Technology, Science and Society, Stanford University, CA 94305.

[†] Experience includes innovative work in jet engine and rocket propulsion; automotive; paper and fiber processes; petroleum; steam and gas turbines and power plants.

I. INTRODUCTION

This paper elaborates an appropriate model for the processes of innovation in industrial societies. It begins with a summary of the present views of such models by economists, sociologists of science, historians of technology, and by working scientists, in order to provide a basis for discussion and comparison with the improved model. The reader should be aware that, owing to space limitations, each of these views will necessarily be somewhat oversimplified.

An argument has persisted for decades in the literature of economics concerning the relative importance of "market pull" (sometimes called "human needs") as contrasted with scientific push" (sometimes called "technological push"). In this context, market-pull is seen as calling forth or inducing new inventions, products, and processes by market demand. Scientific push is seen as innovations arising from the supply side, that is, from the creation of new knowledge in the form of science and/or inventions.

Most economists who have taken sides in this discussion appear to favor "market pull", sometimes seemingly to the exclusion of other forces. An often cited example is the paper of Myers and Marquis (1969). The extensive work of Jacob Schmookler (1966) correlating inventive activity and economic opportunity is also sometimes cited as supporting the "market-pull" view, although Schmookler himself drew much more limited and carefully bounded conclusions. Mowery and Rosenberg (1979) provide a thorough critique of the basic papers advocating market pull and conclude that they do not make an adequate case. More specifically, Mowery and Rosenberg make a convincing argument that these papers confuse some necessary conditions for innovation with the totality of sufficient conditions, and hence that arguments advocating primacy of market pull are basically flawed.

There is another strong argument against any model that depends solely on "market pull": innovations are not possible until the supporting technical base in devices, knowledge, and process are ALL available, or can be created, at the next step. Consider, for example, the desire and market pull for flying machines; such desires and hence market pull existed and were documented in ancient Greece. But even in the Middle Ages, the lack of adequate materials, power-plants, and the knowledge of dynamics necessary to create adequate in-flight stability made flying machines technologically impossible. Repeated attempts to fly testify to the existence of a "market pull", but anyone knowledgeable in the technical bases of aircraft would have to expect them to have failed—as they regularly did. The same remark still holds for commercial fusion power: we do not yet know how to create it, even though a strong market pull for inexpensive energy exists. We must conclude that no model based purely on market pull will be adequate.

A different version of this discussion has occurred in the technical community. Some scientists have argued that science is the primary driving force for innovative activity. This point of view has seldom been the basis for published journal articles, but the present writer has heard scientists take this view many times in public speeches. In a famous paper on the origins of modern innovation in the German dye industry, J. J. Beer (1959) argues that the key factor is the "application of science to industry". Many modern commentators follow Beer and call innovation "applied science". Enough doubt existed on the role of science that both DOD and NSF carried out extensive studies aimed at deciding the importance of scientific research in industrial and military innovation. These two studies, "HINDSIGHT" and "TRACES" used different methodologies and reached somewhat different conclusions. However, both argue for the efficacy of scientific research as an important factor in innovation, and thus implicitly argue for the dominance of "scientific push" among innovative forces. A more detailed critique of these two studies that is relevant to this context is given by Mowery and Rosenberg (1979).

A third view is given by sociologists of science, who have collected many case histories indicating that the predominant source of industrial innovations, at least in terms of numbers, is immediately preceding technology (where technology is used to denote hardware and process). See particuarly Pacey (1975). In addition, historians of technology have repeatedly documented that industrial innovation need not wait on science, but will often proceed empirically when knowledge is lacking. Pacey (1975) and Vincenti (1984) have given particularly detailed and convincing demonstrations of this point.

Nor does the view that science is the primary force driving industrial innovation square with the major outlines of the history of industrial societies. All through the 19th century, Europe, with Britain, held a commanding lead in science, but it was just during this period that the United States rose to be the world's leading power in terms of industrial innovation and industrial output. Since World War II, the United States has held a leading position in science, but during this same period both Europe and Japan have caught up in many areas of industrial innovation and production, and are now leading in at least some areas. Leading science does not correlate one-to-one with leading production. Nevertheless, it is obvious on the face of it that modern science has somehow played an important role in industrial innovation during the past century.

In sum, various communities of scholars have very different ideas about the sources of industrial innovation, but each of these ideas seems insufficient to explain the totality of known effects. When this much disparity continues to exist among serious students of a given subject, one ought to suspect that the difficulty rests on lack of clarity in the underlying concepts and hence in the way in which questions are framed. In the case of the sources of industrial innovation, the lack of clarity seems to be based primarily on the IMPLICIT USE of an inappropriate model of industrial innovation processes. More specifically, all of the viewpoints mentioned above, and the many studies they have entailed, seem to rest IMPLICITLY on what Price and Bass (1969) called the "linear model".

In the words of Price and Bass, the linear model views innovation as "an orderly process, starting with the discovery of new knowledge, moving through various stages of development, and eventually emerging in a final viable form." This linear model* has not been made explicit as a diagramatic model in any publication the writer has been able to find. Indeed, many of the authors of the suggestions cited above, including Price and Bass, are quite clear that they see the linear model as far too simple to be adequate. However, the linear model continues to underlie the thinking in many current speeches and much writing. For example, the linear model is implicit in the argument about technical push as opposed to market pull: to have a push or pull implies a process with a beginning and end and some kind of direct connection between them. The common current name for innovation processes, R&D, also implies the linear model: the phrase itself suggests a direct and unique path from research to development and product. This continued use of the linear model very probably arises from the fact that no other model has been available--discussions cannot proceed without talking about something.

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*The word "linear" is to be understood in this context in the sense of sequential and not in the sense of linear proportionality.

The following section, accordingly, presents a diagram and discussion of the "linear" model and also an improved model called the "linked-chain" model, so that explicit comparisons of the accuracy and utility of the two models can be made.

II. EXPLICIT MODELS

The Linear Model--Figure 1

Figure 1 shows the words of Price and Bass in schematic form. Several implications will be important in later comparisons.

First, Fig. 1 shows a unique, linear pathway from science through development to production and finally to marketing. Second, the flow of innovation is visualized as a one-way process. Third, the only initiating step is research, that is, in Price and Bass' words, "new knowledge".

The model of Fig. 1 is so oversimple and inadequate that its use must seriously distort thinking about processes of innovation. Probably no student of innovation processes believes fig. 1 is a fully adequate model. The specific nature of the difficulties in thinking caused by use of Fig. 1 can be seen far more clearly by comparison with the improved model described in the next section.

The Linked-Chain Model--Figure 2

The elements of the linked-chain model, are shown in Fig. 2. Each of Fig. 3 through 7 elaborates one important PATHWAY for innovation processes among the elements shown in Fig. 2. Some elements in Fig. 2 appear to lack widely accepted names; these elements are defined where they first appear in the discussion. The complete set of elements in Fig. 2 with the five pathways will also be called "R&D".

1. The "Chain-of-Innovation": Pathway One

The first and central pathway in the R&D matrix is shown in Fig. 3. The term **"innovation" is used in the sense of economics, that is, as the set of actions that leads to actual adoption in practice of a device, machine, process, or system.**

The chain-of-innovation most frequently starts with "market finding", that is, with an assessment of what might improve a given product or system, or provide a new product or system that meets an unfulfilled market (use). In the current era, this market finding is usually explicit. Explicitness is not critical; the existence of a market is. This follows from the fact that, if no market (or use) exists for a product, innovation, by definition, will not occur. Innovation (unlike research or invention) is tied from the outset to potential uses and/or potential markets. Innovation does not exist in vacuo. Consequently, innovation implies specific goals (also called design criteria, market conditions, product specifications, etc.). The existence of goals does not imply that innovation is necessarily good or even desirable; it does imply that there has to be some use. A great deal more could be said about "market finding"; however, such a discussion is left for other treatment in order to focus on the central features of the model under discussion.

The second element is some form of what I will label "synthetic design."* There are three important forms of synthetic design which I will label: (i) invention; (ii) preliminary design; (iii) analytic design. Each of these is described briefly since there seems to be no adequate labels and little understanding of processes (ii) and (iii) outside some special branches of engineering.

Invention is well understood in the common wisdom. The patent office defines an invention as a new design sufficiently different from prior art so that it would not have been obvious to an individual skilled in the prior art. For the moment, invention needs no further comment.

The label "preliminary design" does need explanation. It is a very important and common function in modern engineering, but has had no common name, is little understood, and is rarely mentioned in the literature of economics or in discussions of research management. By the phrase preliminary design, I will denote the creation of a new design, a new synthetic arrangement of the product, process or system created largely from old components or items well within the existing state-of-the-art. These old components may be from the prior model, they may come from suppliers of parts (old or improved versions), or they may be designed ad novo using materials and processes already well known. The preliminary design of a complex system is not yet the final, detailed mechanical design for manufacture of parts; it is rather a general layout of the system with only enough detail and supporting computations to make sure it is viable. Old components will not need much study; new ones may require consideration in depth. An example may help clarify.

Let us suppose one is selecting a source of power (prime mover) for a certain task, say, to run a very large air-conditioning system. One might select today any of the following: an electric motor, a gas turbine, a water turbine, a natural gas engine, a gasoline engine, a diesel, a steam turbine, or other possibilities. The decision will be made on the basis of computations concerning the characteristics needed for the task and the cost. In some cases, no commercial unit will fit the need well. Then specific computation of the performance of several of the more likely types of engines might be made. From these computations, one (or at most two) will be selected for further design studies and possible later manufacture. It is emphasized that these designs are not a full set of manufacturing drawings (that is, a detailed design); such drawings would be far too time-consuming and costly at this stage. Rather, preliminary design is only a "scoping", that is, calculations setting forth the major features of the machine--for example size, speed, torque characteristics, and so on. If a new machine is needed, the long process of detailed design will come later.

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* The semantics, taxonomy and clarity of this section have been markedly improved as the result of comments by Dr. Harold Hall of the Xerox Corporation which are gratefully acknowledged. These changes represent the differences between the Rept. INN-1 (Feb. 1985), and the present version, INN-1B (Nov. 1985).

By the phrase "analytic design" I will denote the use of a model in order to reach optimal, or near-optimal designs under known constraints. The constraints will embody the market requirements converted into specific quantitative form as cost, durability, maintainability, weight, size, ..., and the specific ways these are reflected in the product or process. This form of design has been rare because of the difficulties of carrying out the needed optimal analysis using analytical mathematical techniques. However, the advent of the digital computer has radically altered this process, as it has nearly every other function into which it has been closely integrated. Given the digital computer, it is now often possible to simulate a product or process and to carry out a search for optimal, or near optimal, designs. The success or failure of such simulation depends entirely on the accuracy and computability of the model. However, in many cases, models and computer techniques are now available that allow optimal study of old products. And in many of these cases, studies of this sort are already showing that what had been perceived as well developed products or processes are in fact far from optimal. We can therefore expect to see far more use of this process of "analytic design," that is, computer simulation of the product or process to obtain optimal, or near optimal, designs than in the past. When well integrated with testing programs, the use of such analytic designs can greatly reduce the number of expensive tests required, improve the details of the design study of critical components or functions, and thus vastly speed development, and/or reduce costs while simultaneously creating a better end product. For all these reasons, we must also expect that firms that do not carry out such studies, when they are feasible and not overly expensive, must expect others to do so thereby endangering market share.

I do not want to leave the impression that these three forms of synthetic design are independent, stand-alone processes; they are not. Often one form is best suited to a given component and another to a second component within a single larger design. In other instances, an invention may produce a result that is more than competitive with the current preliminary design of "next year's model" based on changes in parts of an existing model or manufacturing process. Invention may also produce a wholly new design. Thus the three modes of design are both supplementary and competitive not only for given products and processes, but also as means for supplying given functions in the marketplace.

The point that needs emphasis here is not whether a new model is created by invention or an old product is improved or selected via invention, preliminary design, analytic design, or all three, but rather two other ideas. First, new models or new products do not flow directly from research; typically they flow from a synthetic design. Thus research leads to product innovation only insofar as it stimulates a design. Second, design is qualitatively different from research. A design is the reduction to paper (drawings) of the specific features of a real embodiment of a real machine, system, or device. This will usually involve various sorts of computations to support decisions, but the critical end-product is the drawings or specifications for a design.

The design process is inherently inductive and creative; the process: (i) begins in a human mind; (ii) uses a selection, creates new parts, or makes a choice of process among known ideas and available components and parts; and (iii) reaches a synthesis that satisfies a set of known, present criteria (or goals).

Research is usually investigative and deductive; it may also be analytic. Research may use synthesis and/or induction, but often does not have preset functional criteria in the sense of design criteria (see also discussion of Figs. 5,6,7). Research typically idealizes the processes and reduces the number of variables to produce data or conclusion(s) based on an idealized situation. A design must deal with all the important aspects of a real situation if it is to succeed.

Once a synthetic design is believed to meet the criteria adequately, it may be passed along the chain of innovation to another group who: (i) make a detailed design; (ii) manufacture prototypes; and (iii) perform tests. These processes [(i), (ii), and (iii)] together are usually called development. It needs to be emphasized, however, that development far more often than not demands alterations in the original invention or preliminary design.

In an ideal world of omniscient technical personnel, ideas would be successfully embodied in drawings and then in hardware on the first try, and most of the chain from invention to production would be unnecessary. One would go from concept to drawings to production in a steady, linear chain. In the real world, such a linear process seldom, if ever, occurs, particularly in high-volume production. The question is not whether problems will occur during invention, development, and initiation of production, but rather whether they will be solved at each step. Consequently, tight feedback links among the elements of the chain of innovation are extremely important, since knowledge developed in earlier stages must be available in later stages.

In practice, the various steps in development often take considerably more time and money than the basic research. Steiner (1982) says the assembly of a body of knowledge "tends to be the longest, most difficult, and least understood" phase of the total task in developing a new aircraft. (See also remarks regarding process and systems engineering in the discussion.) Drucker (1984) states that it takes 20-30 years for a new product or a new concept to become mature and fully utilized, and he makes a strong case for the point through his examples. Kline (1972) points to the same kind of time scale in research.

Little needs to be said about either production or marketing here, because these functions are relatively well understood within the common wisdom. However, one further comment is needed concerning the nature of the elements in the chain of innovation.

The steps shown in Fig. 3 are the longest chain one normally finds, and are typical of heavy industry (such as those in the automotive or power-generation industries). In the production of simpler devices or of devices that are only a small variation from prior models, some steps may be jumped, and the chain will appear shorter. This difference in the number of steps in the chain of innovation is highly dependent on the time-scales required for product development and the costs to the manufacturer and user of failures in service. Hence there is a large variation from one industry to another.

2. Feedback Links: Pathway Two

Three types of feedback links essential to effective innovation in the current era are shown in Fig. 4. The first type of feedback is shown as circles linking the stages along the central chain-of-innovation. The knowledge developed in early stages of work along the central chain-of-innovation is usually critical to success in later stages. For this reason, some companies have a team of people pass through the various stages of a given project until it reaches the market. In some companies, however, the work is passed from one group to another at the interfaces along the chain-of-innovation; in this pattern of work, the feedbacks shown as circles on Fig. 4 are particularly important.

A second type of feedback is shown as thin arrows beneath the chain-of-innovation on Fig. 4. The thin arrows indicate the improvements in a given product that arise from deficiencies discovered in service, for example, a replacement part in a given automobile model. The needed changes may require work in any or all of the prior stages along the chain-of-innovation, and hence arrows to each prior stage are shown.

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A third type of feedback is shown as a heavy arrow leading back to market-finding on Fig. 4. The heavy arrow indicates assessment of product utility and competitiveness that inevitably is part of the planning and design of later models or new systems for a given purpose.

All these kinds of feedbacks are normally present in modern industrial practice. Organizations that are repeatedly successful in innovation, such as General Electric and the Bell Labs, have typically given much thought to making these feedback links work effectively.

3. Connections to Research Through Knowledge--Pathway Three

The third pathway in the R&D Matrix is illustrated in Figs. 5a and 5b. These connections to research lie outside the processes implied by the linear model, Fig. 1. Consequently these processes have been relatively little understood and have seldom entered public discussions of current innovation processes[†]. They are, nevertheless, the normal, common connection between the chain-of-innovation and research. To understand these connections, it is necessary to be clear on the usage intended for the word "science" and its relation to the sectors labeled and "research" and "knowledge" in Fig. 2.

[†] An exception is the recent paper by my Stanford colleague W. G. Vincenti (1984).

I take **science** herein to denote: "The discovery, creation, verification, organization, storage, and dissemination of truth assertions about physical and biological nature." Other definitions of science are possible and useful in other contexts, but this one will serve to clarify the matters under discussion. Under this definition, science includes much of the content of the two layers labeled research and knowledge in Fig. 2. Under this definition, science also includes activities not contained within the layers labeled research and knowledge—for example scientific education and the organization and socialization of professional scientists.

Conversely, research as we understand it includes elements beyond science in the sense just defined. While much industrial research is science in the sense of this definition, we shall also need to consider other forms, including systems research, research on processes, and research in organization of production or quality of product. The various forms of research that relate to production lie at two places in the diagram of Fig. 2.

In the present model, research which is also science (under the definition above) lies primarily in the two top layers of Fig. 2. Research is a process, or more precisely, many processes. Research is not in itself knowledge, although it produces knowledge. Knowledge is not and cannot be a process; **knowledge** is a property--a kind of quantity that exists and can usually be stored for future use.[*] Processes are actions running through time and cannot be stored. For this reason, Fig. 2 shows two layers lying next to the central chain of innovation — research and knowledge. The two layers together constitute a central part of what we usually call "pure science". The layer labeled knowledge is purposefully placed between research and the elements in the chain-of-innovation because knowledge intermediates between research and the processes of the central chain-of-innovation. This intermediation must be firmly grasped and constantly remembered if clear understanding is to be obtained concerning the principal connections of research to innovation.

Figure 5a shows the common type of linkages between invention, knowledge, and research. Suppose I have in mind the creation of a new product or an improved version of an existing product. I need to understand its physical and/or biological processes, particularly those portions of the processes that may create problems in operation. In thinking about these processes and problems, I draw first on what I already know, on the science I learned in school or elsewhere, on my experience and knowledge of the product involved, my thoughts about the new versions of it, and also on my knowledge of what the product must do in the market and the costs it must reach if it is to be successful. Frequently, I will not have enough information about some particular problem. A real example may help clarify. Suppose I am inventing a fuel system for an automobile engine in which I hope to reduce production of pollutants. I shall need to understand the interaction of mixing processes in turbulent flow and droplets of liquid gasoline. Since I lack enough knowledge about that problem to fully analyze the system I am thinking about, I shall do a literature search. I shall find, in this case, that the literature is inadequate for my purposes. Next I shall talk with leading experts. In this case I would find that they also do not have enough information. At this point, a call for potential research is activated. If the research is successful and the results come to hand, some years downstream in time, we shall publish the findings, and we and others will begin to use them to design improved fuel systems and in other inventive situations as well.

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[*] Whether all knowledge or only most forms of it are "storable" is arguable. Losses of knowledge via forgetting, decay of records, and "disproof" also occur. the important point here is that knowledge and research are fundamentally different in character.

These processes are illustrated by the links, arrows, and small circles in Fig. 5a. The first step is a call on knowledge indicated by line 1 linking the process of synthetic design (invention, e.g.) to the small circle labeled "K" in the knowledge sector. If that provides the necessary data (or theory, or concepts), the information is taken back into the inventive or design process and used. This return link is labeled line 2 in Fig. 5a. If I do not find the needed information in any of the various sources of knowledge, then I may activate research; this link is the line labeled 3 from the circle "K" to the circle "R" in Fig. 5a. Since it is problematic, it is shown as a dashed line. The return line from research, some years hence, is the line marked 4 in Fig. 5a.

A very similar set of connections exists for the processes called: detailed design, test, and production. These connections are shown in Fig. 5b. In each case, we first call on the cumulated human knowledge to solve problems. In each case, this call on knowledge tends to highlight outstanding problems or gaps in collective human knowledge about physical and biological behavior. Thus, the questions that are thrown up by the processes of invention, preliminary design, analytic design, detailed design, failure in testing, or difficulties in production processes in effect define research problems. These problems are by definition applied; they arise from concern with present or future products for use. However, this does not mean the research generated will necessarily be less long-range, less significant, or less fundamental than what is called "pure" research. A significant fraction of the most important advances in science and in mathematics arose historically from consideration of very practical problems. A number of examples are given in Table I, Section D. Four are mentioned here to make the point clear:

- The first derivation of the Second Law of Thermodynamics in the 1820s by Sadi Carnot was an explicit result of Carnot's recognition that, while steam engines were rapidly changing the societies of Great Britain and Western Europe in fundamental, important ways, no one understood the limits on the efficiency of such engines.

- Edison paid a mathematician to work out the mathematics of the parallel circuit as part of his development of electric lighting systems, because without that theory the system would have been far too expensive, and demanded far too much copper.

- The genesis of the field of mathematics now called "asymptotic perturbation theory" was a paper by Ludwig Prandtl in 1904 in the course of providing a direct solution for important problems in the aerodynamics of wings and other related applications.

- The first work in probability theory was done by the Marquis de Laplace, who was concerned with calculation of odds in gambling games.

These examples make clear that the importance of research is not highly correlated with its "purity". A distinction that is important, however, is whether the research is long-range or short-range, since typically industry is more effective on short-range and universities on long-range problems. See Kline (1972).

It is important to note the types of research connecting the different stages in the chain-of-innovation to research and knowledge. Invention often gives rise to problems in what we usually call long-range science. Detailed design and testing more often give rise to what is often called "systems research", but may also give rise to long-range science. Production (and therefore also design for production) on the physical side gives rise to what is usually called "process research", that is, on the ways that physical processes can be altered to improve the quality of products or the efficiency of production for a given quality of product. Product or process failure gives rise to any or all of these forms of research.

Process research, whether on the physical or social aspects, or both, is often the most effective type of research in producing rapid effects on corporate profits, because improvements immediately affect competitive position and profit margin. Moreover, process research is the central type of research for any industry concerned with the production of materials, for example, steel, non-ferrous metals, plastics, chemicals, cement, tool-steel, semi-conductors, etc. New applications in such industries may arise from other sources, but the production itself is primarily connected to process research.

The physical researches arising from invention, preliminary design, testing, production or product failure are typically much like physical or biological science. What is usually called market research is also important, but is a type of work so different, qualitatively, that links to research are not shown in Fig. 5b, to avoid confusion.

Finally, in connection with the links shown in Fig. 5b, we need to note that any information disclosed by such researches adds to our stock of knowledge, and, provided only that it is disclosed (rather than held as trade secrets), it adds to the cumulative knowledge of the human race and can be used on the next set of problems, as noted in the example about turbulent combustion given above. The example appears to generalize.

4. Direct Connections between Research and Innovation--Pathway Four

In addition to the connections shown in Fig. 5b, there is an important, direct pathway between scientific research and the chain-of-innovation. However, this pathway is significantly different in two ways from the processes implied by the linear model of Fig. 1.

First, the link from scientific research to development seldom can be immediate; it must almost always pass through some form of design process. One cannot develop what has not been designed and built. Design demands the mental creation of a specific product, an embodiment in hardware. Scientific research does not usually produce such an embodiment. Hence an intermediate, synthetic design step is usually necessary; however, this step is usually omitted in the linear model.

Second, the connection to invention is a two-way street. This applies to the direct link as well as the link through knowledge discussed in connection with Fig. 5a.

Figure 6 accordingly shows a two-way arrow connecting research to invention. The two-way arrow indicates not only that long-range science creates opportunities for new products, but also that perceived needs, or possible market advantages, can stimulate important researches. Such research need not necessarily be any less long-range or fundamental because it arises from a need that was recognized in advance, as already noted. (See Table I for examples.)

5. Direct Connections between Products and Research--Pathway Five

The fifth and final pathway in the R&D matrix is from market to long-range research. This pathway can be seen as a direct connection, or as tracing back through market-finding, as shown by the two arrows on Fig. 7 marked I_1 and I_2. In the present era, many government agencies and large corporations continually ask the question, "What areas of long-range research are likely to potentiate inventions and hence new products, or to suggest advances in the quality or performance of old products?" Thus market factors or military needs can stimulate long-range research. An example is the development in the Bell system of the silicon semiconductor (as a result of known long-range system needs for a solid-state amplifier). Current emphasis in some government agencies and in the auto companies on long-range research in turbulent combustion are motivated by well-known design and performance problems in jet engines and auto engines, respectively. Numerous other examples exist.

A second important, qualitatively different link also exists between market products and research. The production of new instruments, /tools, and processes has in many instances made possible new forms of research. Galileo's foundation work in astronomy became possible only after the telescope became available. Pasteur's discovery of microbes awaited the microscope. In the past decade, the procedures and equipment for radioactive dating have allowed major advances in understanding the evolutionary origins of homo sapiens. Many other examples are given by de Solla Price (1984), who sees the development of such new instruments and processes as often "determining what was discovered."

Even this paper by de Solla Price does not put the case strongly enough. Most of what we now call "advances in science" depend as much on modern sociotechnical systems as on prior science. A large fraction of the work requires modern tools, components, computers, techniques, etc. In some cases it also requires the infrastructure of the large laboratory. Indeed, the modern research laboratory would be impossible without modern sociotechnical systems--both the technical and the social parts.

For these reasons, the dependence of science on technology is shown as a separate link marked "S" on Fig. 7. The SUPPORT FOR SCIENCE link (S) follows the same route as the INITIATION OF SCIENCE link (I_1) in Fig. 7., but the two links represent qualitatively different functions.

The new science, accumulated with the aid of the link S, will, after some time, aid in further technological innovation. Thus science and technology continuously assist each other through multiple links along the chain-of-innovation. The linear model of Fig. 1 creates not one but many significant distortions of the reality.

I do not want to leave the impression that the links from market is the only major source of long-range research. Long-range research, as science, has always had two major, identifiable sources and motivations: (i) the desire to solve problems in the real world (market) and (ii) the state of science itself, the questions thrown up by the existing state of data, concepts, theories, discussion and debate. Often (i) and (ii) operate simultaneously. Few large scale human activities are singly motivated.

IV. DISCUSSION

A. Examples of the Pathways

The previous section indicates that there are five important pathways within the R&D matrix, rather than one. It is important to be sure that all five are significant pathways. Table I provides this assurance by listing ten important historical examples that arose along each of four pathways. Each of the four lists could be extended by anyone familiar with the history of technology.

The fifth pathway constitutes the feedback links shown in Fig. 4. As indicated in discussing Fig. 4, these links are an inherent, necessary, everyday part of innovation in industrial societies.

Hence, Table I, with the discussion of Fig. 4, makes the important point —all five pathways are important. No one pathway describes all the sources of innovation or all the necessary functions for successful R&D in industrial societies.

It is more difficult to be sure that these five pathways cover all the important functions and sources of innovation. Comments from several dozen colleagues suggest that at least no very obvious pathways have been overlooked.

B. The Role of Individuals, Institutions

To this point, the discussion has centered on schematics and the pathways within the R&D matrix. This could lead to the idea that innovation can be done by routine bureaucratic delegation. This is usually not the case. As noted by Schon (1969), resistance to change must be expected in human affairs, particularly within large institutions. Important innovations therefore need individuals with vision, knowledge, influence, and much persistence—individuals Schon appropriately calls "champions". Unless the institutional culture at least condones such champions, important innovations will rarely, if ever, occur. This point is elaborated very effectively by Peters and Waterman (1982). More generally, the climate of the institution, whether large or small, must encourage and foster innovation, or it is unlikely to occur.

C. Reference to Wider Contexts: R&D as Sociotechnical Systems

This article concentrates on the internal form of R&D systems and how this form can be better understood; space does not permit full discussion of the external environments nor the social effects in such systems. However, certain basic facts about the impact of R&D since 1830 are essential to understanding some of the material that follows.

The physical bases of all human societies, past and present, are sociotechnical systems. These are organized systems with strongly coupled social and technological components. We humans use two types. In the first type, we manufacture hardware. We then diffuse this hardware into sociotechnical systems of use in order to accomplish tasks that lie beyond unaided human powers. It is this extension of human powers through sociotechnical systems that has made homo sapiens the dominant species on earth.

Purposive innovation in sociotechnical systems by humans is not new; it has occurred for millenia. Indeed, purposive innovation in sociotechnical systems, coupled with the transmission of the changes to oncoming generations via culture, is what probably distinguishes humans most clearly from other animals. What is new is the existence of special systems for creating these innovations—what we now call R&D. R&D systems are only about a century old, but they have caused a radical change in the human condition and the rate of change of civilization, as will be explained further below. Further elaboration of the ideas in this section are given in Kline (1977).

D. Some Implications of the Chain-Linked Model

1. The Bases of Innovation--The Distinction between Total Cumulated Knowledge and This Year's Research Output

Few people familiar with modern history would deny that research in the physical and biological sciences has had an enormous impact on human societies and human life styles. Research has potentiated many radical developments in the technical side of sociotechnical systems, which in turn have vastly altered human power and hence also human social systems. Examples include the radio and television, computers, laser surgery, plastics, electric power and light, atom bombs; the list has now become so long it is not easy to enumerate.

At the same time, we have seen that research is not the DIRECT source of innovations, and that much innovation proceeds with little or no input from current research. This seeming paradox is easily resolved, once we see clearly the relation of research and knowledge to innovation, as illustrated in the chain-linked model of R&D processes.

Referring again to Fig. 5b, we observe that the primary input, and the first line of reference, for innovation processes everywhere alo g central chain-of-innovation is not research but the TOTALITY OF CUMULATED HUMAN KNOWLEDGE. Moreover, the relation between research and the totality of cumulated human knowledge is an integral one. Each bit of research adds a tiny increment to the totality of human knowledge. To put this in a metaphor, the totality of human knowledge is like the total stock of human housing, and research is like this year's addition to our housing. No one would expect us to replace all our housing this year. However, the linear model of Fig. 1 implies that we replace our cumulated knowledge with this year's research output whenever we begin innovation. This implication is probably the most important cause of confusion resulting from implicit use of the linear model of Fig. 1 to think about innovation. Some further comment is therefore appropriate.

Any modern technical person beginning a task in innovation will not turn first to research. On the contrary, one turns first to the current state of the art, then to personal knowledge about the governing principles of the field. After that, one goes to the literature, consults colleagues, calls in leading experts. Only when all that does not suffice, does one start research. Even then, many innovation projects we now attempt routinely would be not only unfeasible but would be literally unthinkable without the vast accumulated storehouse of knowledge attained by several centuries of work by many, many workers in the appropriate fields of research. For example, the design of an aircraft jet engine would be unfeasible and unthinkable without a solid grounding not only in Newton's laws of mechanics and the elaboration of them in fluid mechanics, elasticity, dynamics of machinery, but also numerous other special fields of knowledge that modern technical teams take for granted (e.g., chemistry, thermodynamics, control theory ...).

Over the past two centuries, this cumulation of knowledge about physical and biological nature has provided the human race with an increase of many orders of magnitude in insight into physical and biological nature. Furthermore, we have used this increased and increasing knowledge to vastly improve our stock of tools, instruments, machines, and processes and to build increasingly powerful sociotechnical systems. The result is an accelerated increase in the capability of human sociotechnical systems that began about 1830, and is still continuing. This acceleration has been documented quantitatively by Lienhard (1979) and also by Kline (1977), using somewhat different methods. In many instances, this power of human systems has increased more than a million times during this period, and the process does not yet seem to have ended or to be slowing down in an overall sense.

Given these ideas, it is instructive to return to our jet engine example. Its design and manufacture would be equally unthinkable without these powerful systems, including special manufacturing processes, advanced materials, and skilled, cooperating workmen. The same remarks apply to many modern systems, such as automobiles, air-conditioners, petroleum refineries, computers, steel mills, electric power systems and the appliances they power, and so on. In all these cases and many more, it would be infeasible and unthinkable to design these systems without the foundations of the total cumulated human knowledge of mechanics, electromagnetism, thermodynamics, quantum mechanics, etc. Furthermore, it would equally infeasible and unthinkable to manufacture or operate them without the cumulated power of modern sociotechnical systems.

It is the combination of the increased understanding and the increased power of human sociotechnical systems of many kinds that we now bring to bear on innovation, and that has accelerated the processes of change.

The ideas expressed in this section were directly borne out in project "HINDSIGHT". In that project, the input of research was traced back 20 years, but this time period was found to be seriously insufficient; many of the inputs traced back much further in time to the foundations of the underlying subjects, as was later shown in project "TRACES". This is precisely what must be expected from the discussion of this section.

What, then, is the message about research and innovation? First, research is critical. It is the primary tool we use to create our storehouse of cumulated knowledge, but it is the cumulated knowledge and the systems built from that knowledge that provide the primary DIRECT inputs to current innovation. It is only when that storehouse of knowledge and current systems is insufficient that we turn to the much more expensive and MUCH SLOWER process of research for direct solutions to problems in innovation.

I emphasize the words "much slower" because the matter of time scales is very important. Since we cannot expect the output of this year's research to add a major fraction to our total stock of cumulated knowledge, we should not expect current research to affect this year's innovation greatly. On the other hand, we should expect this decade's research to affect the results of innovation in the 1990's and beyond very strongly. That is what history tells us, and that is what many observers have documented. The time scale of completion of innovation based on new research is about a decade. See, for example Steiner (1982); see also Kline (1972), Drucker (1985).

This distinction is far from trivial. The failure to understand the distinction between cumulated human knowledge and power of human sociotechnical systems and the output of this year's research has caused serious troubles in U.S. R&D systems. More than one large corporation, apparently acting on the implications of the linear model, established large research laboratories in the expectation that they would produce important innovations over short time spans. When this development did not occur within a few years, the laboratories were dismantled.*

2. Undervaluation of Process and System Research

As already noted, system research is the major component of research associated with the development step in the central chain-of-innovation of Figs. 3-7; process research plays a similar role for production activity. To be concrete, a very large portion of the work in the space program has been systems research, including validation of components and coordination of very complex activities. The only kind of effective research that bears directly on any industry producing materials, rather than products, is process research. Such industries include ferrous and non-ferrous metals, rubber, petroleum, paper and fiber, and many others. The importance of systems and process research needs no belaboring.

However, the linear model of R&D shown in Fig. 1 has no place for either systems or process research; the model excludes them by implication. As a result, system and process researches appear to have had less attention within the total USA R&D system than they deserve, in several ways. First, they have not often been explicitly discussed or considered in national discussions about "science policy". Hence, research funding has not favored these areas, and the science policy advices to the Congress have not made sufficiently clear the complete patterns of innovations and the associated needs. As a result, university research, which does what can be funded, has neglected process and systems research relative to the other areas. This neglect has, in turn, reduced the numbers of new, educated, technical workers who appreciate the value of these areas and are trained in them.

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*That this did actually occur in one important case was confirmed by a reviewer of the first draft of this article who was involved in the decisions.

Since systems research is essential to reliable performance of products and any gain in process research effectiveness is quite rapidly reflected on the bottom line of corporate financial statements, we should expect corporate managers to continue these activities--as indeed they have. As one reviewer of this paper pointed out, most of the advances in the process and materials industries in the past century are direct results of research on and cumulated knowledge concerning process functions. Despite this fact, appreciable distortion of the system does seem to have occurred from use of the linear model of Fig. 1. An important and rather clear example of this neglect that has become apparent in the last few years is the neglect of the U.S. machine-tool industry, which is fundamental to advances in processes in most other industries. Few universities have offered classes or done research in this area. Innovation appears to have slowed to the point of national concern, as a recent report sponsored by NAE makes clear.

Reconsideration of policy regarding research funding and education in process and systems research seems needed.

3. The Role of Invention and of Design in Innovation

a. The Role of Invention

Invention is an important initiating activity in the central chain-of-innovation. Jewkes et al. (1968), in a study of many cases, conclude that invention was still an important source of innovation in the decade following World War II. Burton Klein, a long-time student of the subject, not only agrees, but reaches a much stronger conclusion. Klein (1977) says,

> "Where do the discoveries come from to make new S-shaped curves? Assuming that the industry has already reached the stage of slow history, the advances will seldom come from major firms in the industry. In fact, of some fifty inventions--most of which were included in the Jewkes, Sawers, and Stillerman study--that resulted in new S-shaped curves in relatively static industries, I could find no case in which the advance in question came from a major firm in the industry. In some cases (Bessemer Steel, the electric steel process, jet engines, the Polaroid Land Camera), the inventions came from newly established firms. In others (the Diesel locomotive, synthetic fibers, computerized machine tools), the inventions came from firms in other industries or from universities. In other words, as long as organizations remain highly dynamic, they can produce a series of important advances —for example, as have been produced by Bell Telephone Laboratories. But once firms in an industry become static, the discoveries will come from newcomers. Evidently the process of going from a dynamic to a static organization is highly irreversible, for otherwise firms that had remained static for a period of years could have made major discoveries."

Thus B. Klein sees individual inventors as playing a critical role in industries that have reached a stage of "slow history", which he defines as the upper part of a major S-curve of growth. It is important to note that Klein does not make this assertion about all industries; he specifically notes that some large organizations, for example Bell Labs, have continued to make important innovative advances for long periods of time. This is true of a number of institutions, for example General Electric, Hewlett-Packard, 3-M, and many others listed by Peters and Waterman (1982).

At the same time, the status of invention and inventors has been very low in the U.S. when compared with that of research workers, at least since World War II. A researcher is seen as a man in a white coat, working in a laboratory, and producing important results. An inventor is seen as a crank who disturbs the social equilibrium and produces little, if anything, that is useful. This relative status has been clearly reflected in Research Management.

A word count of the titles of all articles in Research Management for the period 1975-80, inclusive, is given in Table II. Note that in Table II the word "research" occurs 297 times; "development", 204 times; "invention", 3 times; and "inventors" 4 times. Such a title count is only a rough measure of status, but the count is so lopsided that the low status of invention is quite clear.

Will inventors continue to play an important role in the future? All the commentators on the first draft of this paper who discussed the issue believed that they would. The writer has just had the privilege of reading ten patent applications by M. R. Showalter, an independent analytic innovator (inventor), all on the subject of lubrication and friction reduction. I am as sure as one can be at an early stage that these patents will have a major impact on automotive engines, machine tools, and many other applications. They solve some very old problems and create new advances. Moreover, they were not anticipated by engineers in the industries involved; the patent searches showed remarkably little earlier work on these problems, despite the fact that most of the solutions are clever adaptations of knowledge created by research and clearly recorded in texts and Handbooks, such as Fuller (1956) and the Standard Handbook of Lubrication Engineering (1969).*

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*Neither the space nor the knowledge exists to explain here the failure to do the innovation logically suggested by research in some fields. However, a few provocative remarks do seem appropriate.

For the most part, we do not teach our engineers to innovate or invent. We teach them how to analyze and do research. We do not even give engineering students a clear picture of the importance of invention and innovation; rather most of the time we stress analysis and practice, and for graduate students, research. We imply the model of Fig. 1. As Peters and Waterman (1982) document, large, rigid bureaucracies reinforce these educational experiences. Nor do we teach engineers, or any other students, that sociotechnical systems are the physical foundations of all human societies and that innovation in such systems is a part of our evolutionary and cultural heritage. Students in the social sciences and humanities, who sometimes later run industrial organizations, seem to be told that the whole process is the result of a mystery labeled "Science". In short, we have not been running some parts of our technological system well. We have not even been inculcating appropriate attitudes.

Showalter has also filed patents that involve ideas on mixing in turbulent fluids. In my opinion, Showalter's ideas on mixing are far ahead of the research community at this time, and turbulent fluid mechanics has been one of my special areas of expertise for more than two decades. I expect to see other important ideas emerge from Showalter's work. The day of the independent innovator is certainly not passed. We should expect to continue to see important innovations created by able, independent, technically trained individuals, since they sometimes bring the detachment about a problem that is lacking in the current common wisdom.

Moreover, invention is not a low-brow activity. It is very demanding, intellectual work. In addition, successful invention requires careful attention to details of hardware, manufacturing techniques, and cost control that long-range research often does not. Successful invention in the late 20th century will often demand more theoretical knowledge than in earlier times, but that does not alter the preceding remarks in this section; rather, it reinforces them.

b. The Role of "Design"

In order to understand innovation in industrial societies, it is important to realize that much preliminary design and also detailed design is institutionalized invention. A large fraction of engineers get paid to create the new. The engineering groups in some companies and industries have been very innovative, repeatedly, for long periods of time; see for example Mowery (1981) or Peters and Waterman (1982).[*]

There is an extensive literature created by "engineering designers" on creativity and innovation. See for example, Mann (1965). This literature has discussed models of innovation that look very much like the chain-linked model for a long time. However, these models usually exclude economic considerations, are often rather complex in details, and typically are couched in jargon that only engineers understand. Hence they were not discussed in the introductory section of this paper. This "design" literature does strongly reinforce the ideas expressed in the chain-linked model, and it certainly verifies the point that much engineering design, both preliminary and detailed, is institutionalized invention and/or innovation.

c. Invention and Analytic Design

In the two preceding sections and in the comments about Fig. 3, invention and analytic design have been discussed separately. In reality, the two processes often interact strongly. Moreover, they should probably interact more than has been typical, particularly in the heavy industries. When a good theory exists, it is obvious that an analytic investigation of a proposed design will be much faster and much cheaper than construction and testing. The widespread availability of the digital computer makes available powerful methods for solution of differential equations, and this amplifies the applicability of this remark greatly. A very high fraction of all design configurations are too complex in detail for ready construction of closed-form analytic solutions, but a considerably larger fraction are straightforward applications for digital computation.

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[*] The reasons why some groups have been innovative and others have not lie beyond the scope of this article, but are discussed extensively in many recent publications, including Klein (1977) and Peters and Waterman (1982).

It thus becomes possible to manipulate invention in the computer and also to search for near-optimal solutions in ways that were not possible up to a decade or so ago. It is this fact that makes possible the re-invention of many machines in improved configurations. The lubrication patent applications mentioned in the preceding section are an example of precisely this kind of work. Much more of this type of work remains to be done. However, it demands a clear conception of the proper role of invention and of the combined use of analytic design and invention as the initiating step in the chain-of-innovation.

In sum, some form of synthetic design is the direct, first step in the chain-of-innovation, not research. Generally, research contributes to important innovation only when it stimulates a new design, or invention. If research is to become important in production, it is counterproductive to assign a low status to invention.

For the purposes of this discussion, we therefore need to note three things. First, we must expect important innovation not only from institutions but also from individuals who are largely outside the formal system, and should arrange the system to make both possible. That is why Figs. 3-7 show synthetic design, and hence preliminary design, analytic design and inventions as primary, common initiating steps in the central chain-of-innovation.

Second, the linear model of Fig. 1 omits the various forms of synthetic design as sources of innovation and replaces them with research. If we believe that replacement, we shall be seriously misled about the nature of innovation and also about the appropriate roles for science and engineering and the relationship between them.

Finally, the linear model seriously misleads us on the role of inventors. The linear model omits invention and related design processes and replaces their role with research. It seems likely that this substitution is part of the reason for the present low status of invention and inventors.

4. Technological Push versus Market Pull

The linear model of Fig. 1 has contributed motivation to a long and inconclusive literature in economics discussing the relevant importance of technology (or science) push versus market pull (or "needs") in innovation. If the chain-linked model of Figs. 2-7 is appropriate, the question is essentially irrelevant and should be dropped. Figures 2-7 suggest several sources as initiators of the chain-of-innovation, and all of them include important examples, as Table I forcibly demonstrates. All need to be continued; no single one is THE major source.

Moreover, if, as indicated on Fig. 4, feedbacks are crucial and the loop back from market to need-finding is essential to innovation (as opposed to long-range research), then questions of cause and effect are basically irrelevant. In looped processes, every cause becomes in due time an effect, and every effect becomes in due time a cause. The distinction between pushes and pulls loses essentially all meaning.

E. THE ROLE OF SCIENCE

The preceding sections point to the importance of invention and design processes as initiators of the central chain-of-innovation and suggest a somewhat different role for research than has often been invoked by direct or implicit use of the linear model of Fig. 1. It is therefore important to be clear about the implications for the role of Science.

Research, including the forms of research we call science, have been and remain critical with regard to innovation processes, in two ways.

First, the knowledge cumulated by research, which is to say largely by science, is the foundation beneath the entire structure of modern industrial innovation and the more powerful sociotechnical systems we now employ. As has been stressed repeatedly, the total cumulated knowledge about how physical and biological nature do function is a principal ingredient in innovation. Moreover, when it comes to establishing truth assertions about nature, the processes we have come to call "science" are far more powerful than any methods previously devised by humans--so much more powerful that, to paraphrase Lombardi, "There isn't anything in second place."

Second, the knowledge and methods of science are used continuously in innovation whenever a question arises within any step in the processes along the central chain-of-innovation. If we don't have the information in our heads, we go to the literature of science and related fields. If that fails, we use "scientific" methods to do some form of research to solve the problems. The methods of science and technology largely overlap, even though their goals are different. It is precisely this continuous linking of scientific knowledge and the methods of science to the processes in the central chain-of-innovation that suggests use of the word "linked" in the name given to the model of Figs. 2-7.

Since World War II, the U.S. government has put considerable funds into support of science. Has that support been an economically supportable proposition? The reconsiderations in this paper suggest the answer is an unambiguous, "Yes!" By and large, were we have kept our research active, maintained strong industry-university links, stimulated bright students to enter the field, we have progressed well and maintained strong industrial positions. Where we have let our research and education lapse, we have suffered relative declines in innovation and have tended to lose position with respect to the international competition over the long haul.

Table I tends to confirm this point of view. However, Table I also makes clear that science and research in all forms is only one necessary condition among a variety of sufficient conditions that initiate and drive innovative processes. We should also keep in mind that technology supports science in critical ways and always has, as noted in the discussion of pathway 5 above.

V. SOME REMAINING QUESTIONS

This paper suggests a considerable revision in the model and hence in our views of the processes of innovation. It therefore raises a number of policy questions. Since the objective of this paper is to clarify the model as a basis for thinking, it would be discursive and premature to discuss these policy issues in depth. However, it seems appropriate to pose several questions as part of an agenda for further discussion.

Do we need to change our patterns of engineering education in order to improve our processes of innovation? If so, in what ways?

Can we formalize the processes of analytic invention and coordinate them with analytic design to create more effective procedures for establishing near-optimal designs of both existing and new devices and systems?

What are the appropriate roles for industrial, governmental, and educational institutions in fostering innovation, given the model of Figs. 2-7, and what should the relationships among these institutions be?

How can we recognize and make more effective the important role of individual inventors in the ongoing process of improving of the efficacy of sociotechnical systems?

How can we improve understanding of innovation processes among economists, government officials, and leaders of business?

VI. CONCLUDING REMARKS

The frequent use, usually implicit, of a linear, single-pathway model connecting research to production has often distorted our thinking about processes of innovation and thereby sometimes distorted governmental policy and industrial decisions. The more important distortions seem to include the following:

- Failure to recognize clearly the distinction between "total cumulated knowledge" and "the output of this year's research" as the knowledge base used in innovation processes.
- Failure to recognize that the initiating steps in the central chain-of-innovation are analytic design and/or invention, and that research affects production only as it stimulates inventions or changes in process.
- Frequent devaluation of invention, of process research, and of system research with respect to their importance in the total R&D matrix.

These conclusions raise questions with regard to a number of policy issues and institutional arrangements that have persisted since World War II in the U.S. systems of R&D. These issues appear in need of separate, further study and possible reconsideration.

Table I

Examples Where Various Processes Have Led to Important Innovations and Advances in Knowledge Related to Production and Use

A. Important product innovations originating primarily from invention with little or no input from research other than product development[*]

1. Wheels, gears, cams, levers, escapements
2. Animal harnesses, plows
3. Swords, spears, bows and arrows
4. Dams, canals, locks
5. Thermometers, barometers, pressure gauges, rulers, micrometers, plumb bob, carpenters level, etc.
6. Printing press, linotype machine
7. Textile machinery (several kinds)
8. Machine tools (lathes, mills, grinders, etc.)
9. Steam engines and early forms of steam turbines
10. Diesel and gasoline engines
11. How to put bubbles in champagne

B. Important products originating largely from "analytic design" studies

1. Jet engines
2. Supersonic aircraft
3. Safety glass
4. Hydraulic brakes
5. Gas turbine power plants
6. Modern hydraulic turbines, steam turbines, and pumps
7. Rocket engines for aircraft and missiles
8. Hand calculators
9. Tape recorders, video recorders
10. Hi-fi systems

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[*] Refers to initial innovation; various forms of science and research have played a role in further improvements of nearly all these products.

C. Research that made possible important industrial products, methods, or processes

1. Genetics: interferon and many other new drugs; new processes for production of materials and foodstuffs.
2. Edison's study of gaseous discharge: Edison effect (basis for vacuum tube and hence for all electronics).
3. Mendel's study of genetics: improved foodstuffs, part or basis for item C1 above.
4. Nuclear physics and study of matter: atom bombs; atomic power, semiconductors and hence microcomputers, minicomputers, and microelectronics.
5. Electromagneticism--the Maxwell equations; experiments of Hertz et al. (radio and TV, radar, microwaves, X-ray.
6. Entropy and availability functions (Clausius and Gibbs) (led to modern analysis of power systems, innumerable system studies, many analytic design methods).
7. Laws of thermodynamics (basis of much chemistry and of metallurgy; innumerable advances).
8. Low-temperature studies: superconductors.
9. Set theory—Galois: computer architecture; computer software; many computer solution methods.
10. Chemistry (synthetic fibers and materials, plastics, improved processes in wine and beer making, petrochemicals, etc.)

D. Research in ideas and fundamental scientific discoveries stimulated directly by real-world (including industrial) problems.

1. Carnot's development of the Second Law of Thermodynamics (to explain the theory of steam engines.
2. Pasteur's discovery of bacteria (from problems in the dairy industry and medicine).
3. Prandtl's development of boundary layer theory (from problems of aerodynamics of wings; leads also to a whole new branch of mathematics, asymptotic perturbation theory.
4. The development of parallel circuit theory (part of Edison's work on electric power and light system; essential to the success.
5. The discovery of synthetic dyes and the foundations of the modern chemical synthesis methods (to create more colors for ladies' dresses).
6. Cost accounting (from problems in control of production in military arsenals.
7. Development of metallurgy and strength of materials (from problems and failures in structures and machines by a long series of workers; builds on Newtonian mechanics).
8. Development of control theory and servo-mechanisms (from problems in control of machinery. These ideas later spread into and became important in theory of human communications.
9. Statistics (from problems in agricultural and biological experiments).
10. Probability theory (from questions about gambling games).

Table II

Frequency of Occurrence of Words in All the Titles of Published Articles in *Research Management* for the Years 1975-1980, Inclusive*

Word	Number of Occurrences
Research	297
Development	204
Technology	84
Innovation	57
Science (or scientist)	34
Project (or program)	14
Patents	12
Licewnsing	5
Inventors	4
Inventions	3

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*All titles appearing were read, including the ending sections on reports and other items. No attempt was made to differentiate intended meanings. the raw count of frequency of occurrence is reported.

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Figure Captions

Fig. 1. The conventional "linear model" of the linkage of research to production.

Fig. 2. Elements of the "chain-linked model" for the relationships among research, invention, innovation, and production.

Fig. 3. The central chain-of-innovation in chain-linked model: Pathway 1.

Fig. 4. Feedback links in the chain-of-innovation: chain-linked model: Pathway 2.

Fig. 5a. Connections of knowledge and research to invention, chain-linked model: Pathway 3 (in part).

Fig. 5b. Connections of knowledge to the full chain-of-innovation, chain-linked model: Pathway 3.

Fig. 6. Direct connections between research and innovation, chain-linked model: Pathway 4.

Fig. 7. Connections between market and research, chain-linked model: Pathway 5.

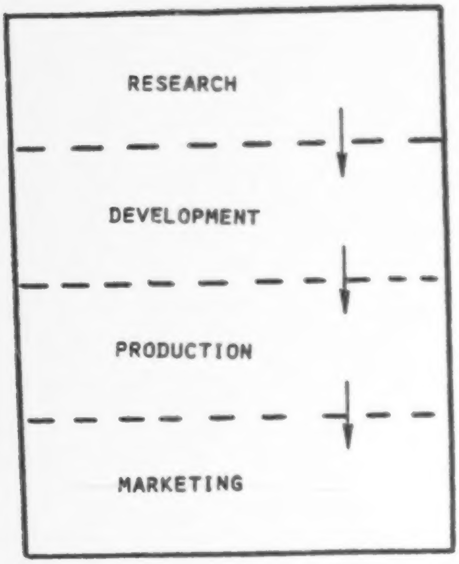
RESEARCH
DEVELOPMENT
PRODUCTION
MARKETING

FIG. 1.

THE CONVENTIONAL "LINEAR MODEL" OF THE LINKAGE OF RESEARCH TO PRODUCTION.

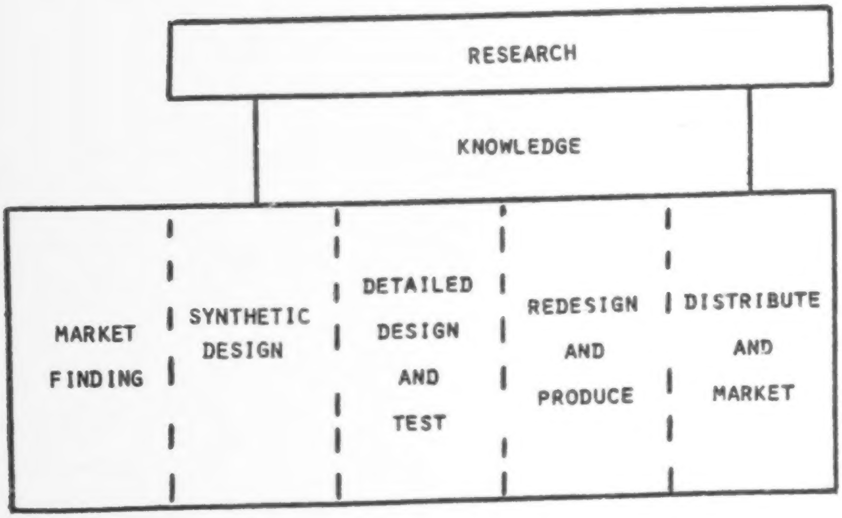
RESEARCH
KNOWLEDGE
MARKET FINDING
SYNTHETIC DESIGN
DETAILED DESIGN AND TEST
REDESIGN AND PRODUCE
DISTRIBUTE AND MARKET

FIG. 2. ELEMENTS OF THE "CHAIN-LINKED MODEL" FOR THE RELATIONSHIPS AMONG RESEARCH, INVENTION, INNOVATION, AND PRODUCTION.

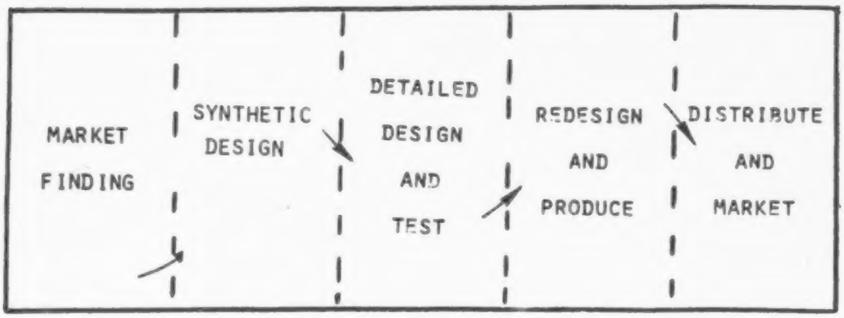
MARKET
FINDING
SYNTHETIC
DESIGN
DETAILED
DESIGN
AND
TEST
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FIG. 3. THE CENTRAL CHAIN-OF-INNOVATION IN CHAIN-LINKED MODEL: PATHWAY 1.

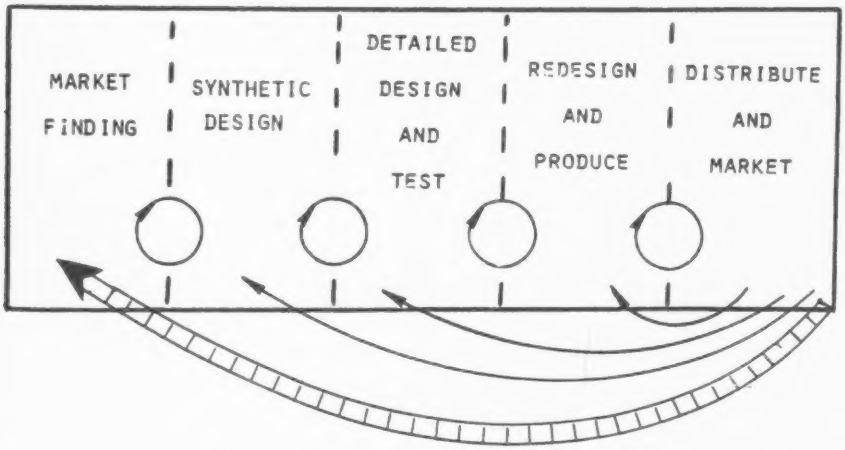
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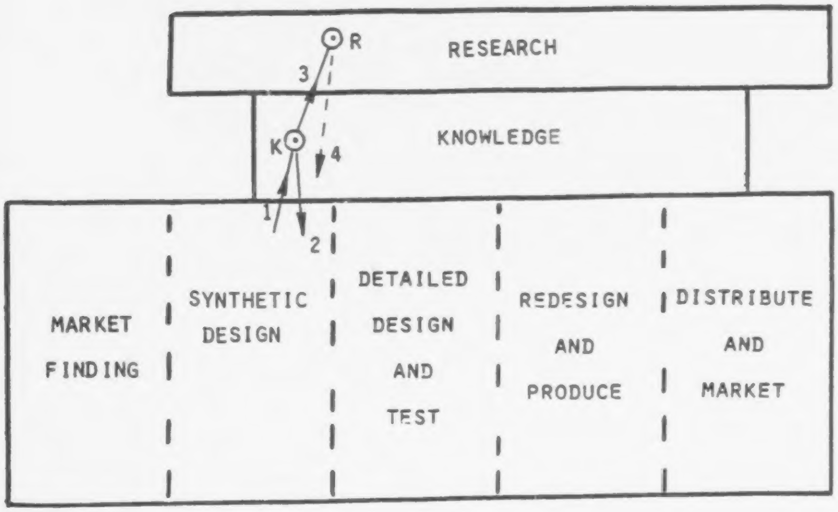
FIG. 4. FEEDBACK LINKS IN THE CHAIN-OF-INNOVATION: CHAIN-LINKED MODEL: PATHWAY 2.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SYNTHETIC DESIGN
DETAILED DESIGN AND TEST
REDESIGN AND PRODUCE
DISTRIBUTE AND MARKET

FIG. 5A. CONNECTIONS OF KNOWLEDGE AND RESEARCH TO INVENTION, CHAIN-LINKED MODEL PATHWAY 3 (IN PART).

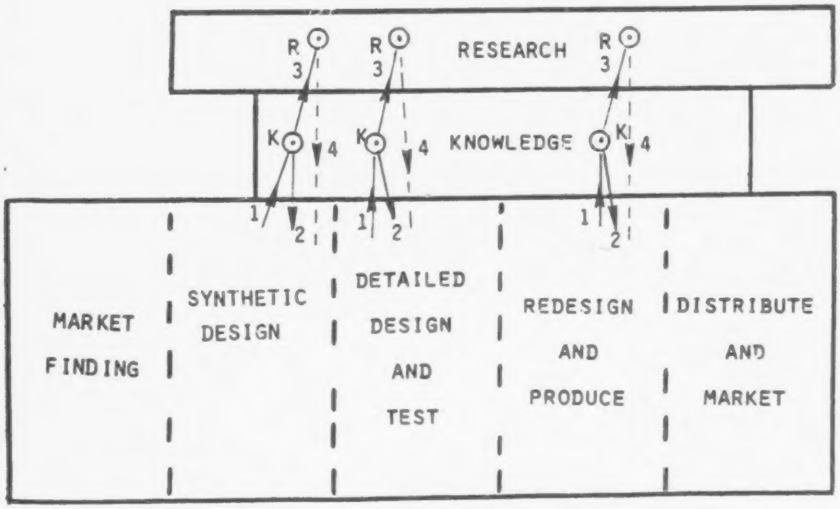
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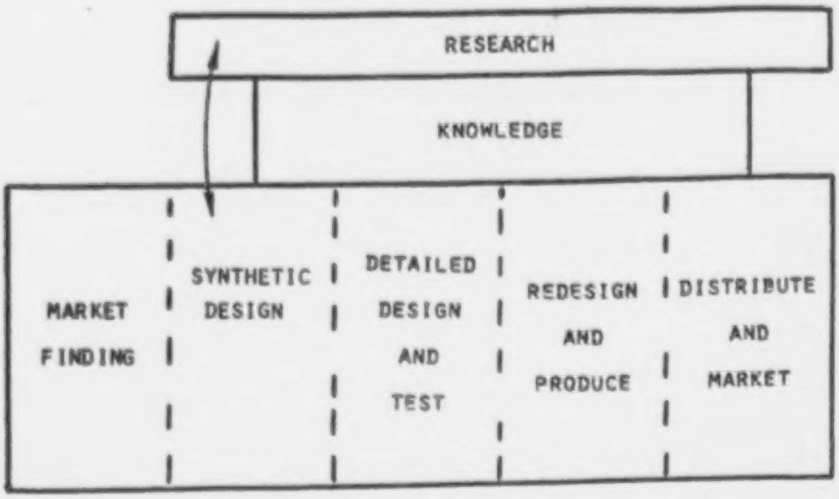
FIG. 5B. CONNECTIONS OF KNOWLEDGE TO THE FULL CHAIN-OF-INNOVATION, CHAIN-LINKED MODEL: PATHWAY 3.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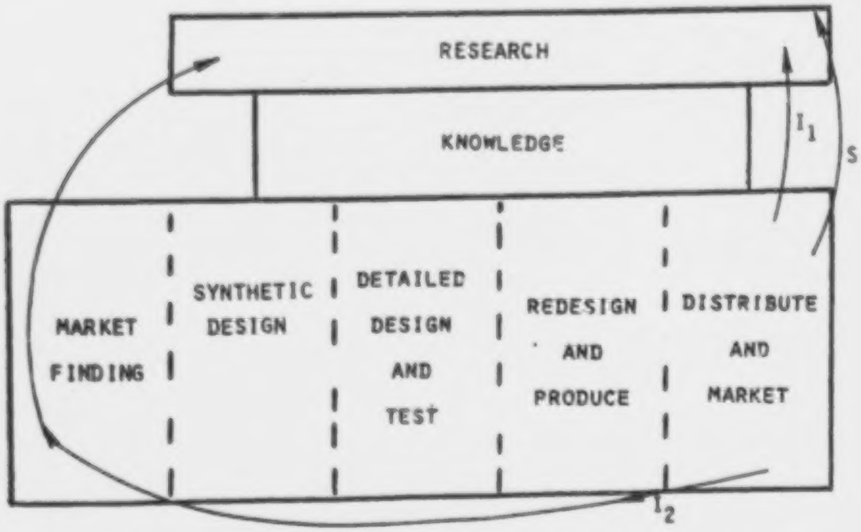
DISTRIBUTE AND MARKET

FIG. 6. DIRECT CONNECTIONS BETWEEN RESEARCH AND INNOVATION, CHAIN-LINKED MODEL: PATHWAY 4.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DETAILED DESIGN AND TEST
REDESIGN AND PRODUCE
DISTRIBUTE AND MARKET
I_2

FIG. 7. CONNECTIONS BETWEEN MARKET AND RESEARCH, CHAIN-LINKED MODEL: PATHWAY 5.

BASIC RESEARCH:

The Key to Economic Competitiveness

by
Erich Bloch
Director
National Science Foundation

Basic Research: The Key to Economic Competitiveness

> On the wisdom with which we bring science to bear against the problems of the coming years depends in large measure our future as a nation.
>
> Vannevar Bush
> *Science—The Endless Frontier*
> July 1945

More than forty years ago Vannevar Bush concluded his famous report to the President with these words. That report noted the contributions that science had made to winning World War II, and argued that the economic battles that lay ahead in 1945 would also require a major effort in research and development if the United States were to prosper.

Science—The Endless Frontier made the case for continuing the wartime effort in basic research through a new agency, and the result was the creation of the National Science Foundation in 1950. The mission of the Foundation was to support basic research and education in the sciences and engineering. The belief was that new knowledge and new researchers would strengthen the nation both militarily and economically.

The case for a strong national effort in research and education in the basic sciences and engineering is as strong today as it was in 1945. We continue to need a strong defense, and we face economic competition to a degree not imaginable in 1945. The need to strengthen our science and engineering base—the collection of people, facilities, and equipment that makes basic research possible—may be even greater today than it was in 1945, because in the past two decades the base has deteriorated markedly.

This booklet summarizes recent trends in research, education, and economics. It makes the case once again that the nation cannot prosper without sustained investment in science and engineering research and education in our universities.

Why Should the Government Support Research?

The federal government supports research in pursuit of three goals:

- Intrinsic intellectual value;
- To accomplish a specific government mission such as defense or health;
- To make the nation's economy more competitive.

The Foundation has always been dedicated to the first goal -intrinsic intellectual value. The nation benefits from significant advances in any field of science and engineering, although the specifics can never be foreseen. The Foundation has always sought to support the most promising work in every field. It will continue to seek excellence in this way.

The second goal accounts for a large proportion of federal support for research and development (R and D). This research may be quite basic in character, but it is driven by the need to accomplish particular missions. This makes the research the proper concern of the agency with the mission responsibility.

The third goal is to perform research that will ensure the nation's economic competitiveness. The need is to do the basic research, and to train the people, that will enable American industry to develop and market products successfully in the international marketplace. This is an area that has had too little attention in recent decades, and one in which the National Science Foundation has much to offer.

The Need for Economic Competitiveness

The world has become a much more competitive place in recent years. The United States was in an economically dominant position at the end of World War II. We had the most advanced technology, and we had the manufacturing and marketing resources to dominate international trade.

That favorable position, however, is gone. The developed nations rebuilt their industry after the war and greatly increased their investment in research and education. And in recent years a number of the developing countries have also become serious competitors.

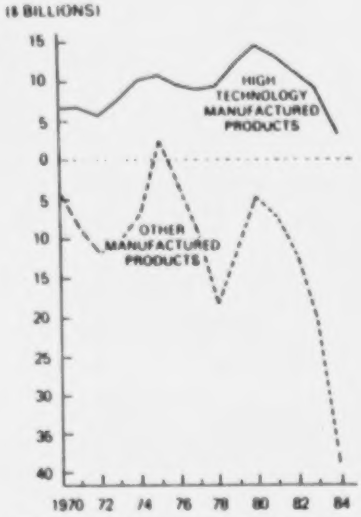
Consider these facts:

Fig. 1
- Our trade balance is overwhelmingly negative, and becoming more so. Even the advantage in the high-technology industries that we once took for granted has virtually disappeared.

Fig. 2
- To be economically competitive we must have high productivity. But in recent years our productivity increases have averaged a scant 0.3% annually. Our competitors have done five to fifteen times as well.

Figure 1.

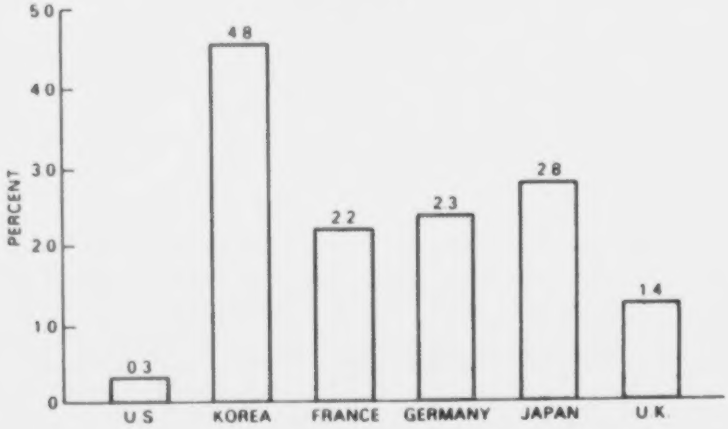
U.S. TRADE BALANCE IN HIGH-TECHNOLOGY AND OTHER MANUFACTURED PRODUCT GROUPS, IN CONSTANT DOLLARS



Source SCIENCE INDICATORS THE 1985 REPORT National Science Board National Science Foundation, 1986

Figure 2.

AVERAGE ANNUAL PRODUCTIVITY GAINS, 1973-83

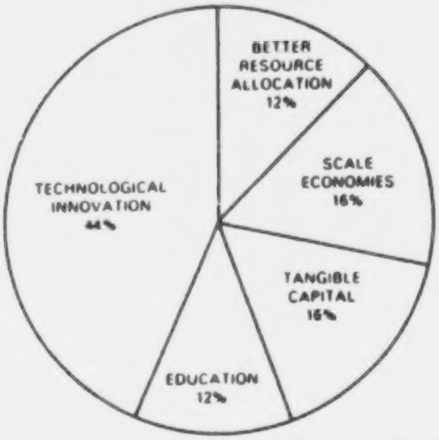


Source U S Department of Labor Bureau of Labor Statistics May 1984

Fig. 3 • Since World War II, new technology has been responsible for nearly half of all productivity increases - more than those due to more capital, more education, or any other single factor.

Figure 3.

CONTRIBUTORS TO U.S. PRODUCTIVITY INCREASES



Source Edward Denison Accounting for United States Economic Growth, 1929-69, Brookings Institution 1984

In recent years the markets that we have to sell our products in have become international; we no longer have the luxury of a large domestic market that our own industries dominate. This is true for both high- and low-technology industries, both computers and shoes. Success in this situation depends on having advanced products, and also competitive pricing, which depends on production efficiency.

Our competitiveness can be improved by developing automated production systems that will give our industries an important advantage. Doing it this way - by improving our own performance through research - is far preferable to relying on trade barriers or other protectionist measures.

Are We Doing Enough Research?

Any society that wishes to remain competitive in the modern world must do three things:

- It must support basic research adequately;
- It must educate enough new scientists and engineers; and
- It must invest sufficiently in research facilities and equipment.

Our record in all three areas is less than it should be. Consider first our level of effort in R and D:

Fig. 4 • The United States has not invested in R and D in recent decades at the rate that sustained growth in a modern society requires. We have slipped from the position of leadership that we held twenty years ago, while our competitors have been pushing ahead in an effort to challenge and surpass us in key technological areas.

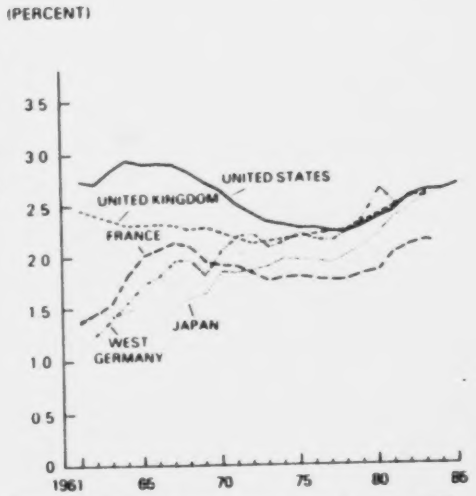
Fig. 5 • Furthermore, the proportion of U.S. federal research support that goes for military purposes is high and rising.

At one time defense oriented research had major positive effects on the civilian economy, because the military was interested in technologies - such as computers, semiconductors, nuclear power, and rocketry - that were more advanced than anything in the civilian sector. This is no longer true to the same extent, however, because in most cases civilian technology is now more advanced than that used by the military.

Fig. 6 • When military research is eliminated from the comparison, our effort in R and D is significantly less, as a fraction of GNP, than the effort made in Japan and Germany.

Figure 4.

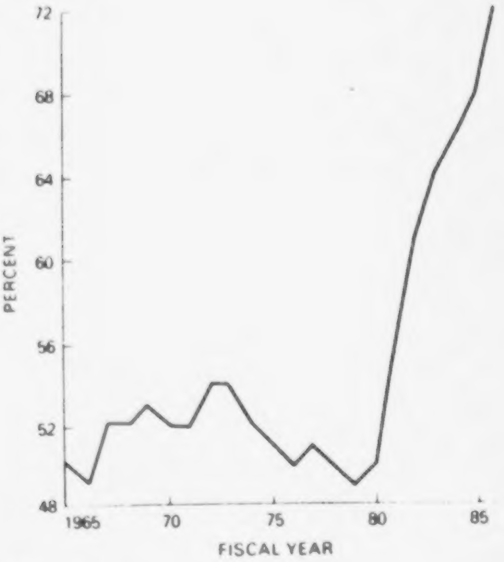
NATIONAL EXPENDITURES FOR PERFORMANCE OF RESEARCH AND DEVELOPMENT AS PERCENT OF GROSS NATIONAL PRODUCT BY COUNTRY



Source SCIENCE INDICATORS THE 1985 REPORT National Science Board National Science Foundation 1986

Figure 5.

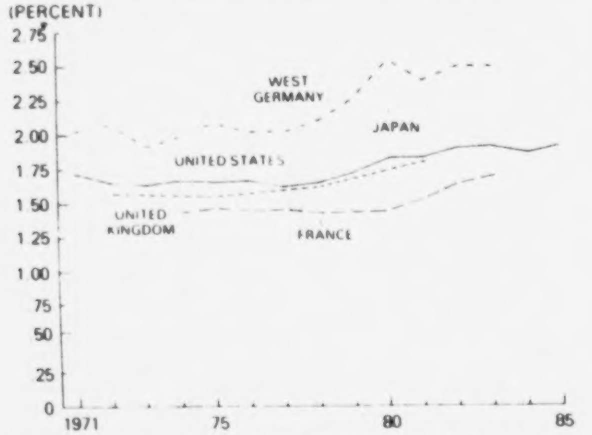
MILITARY R&D AS A PROPORTION OF ALL FEDERAL R&D



Source: Office of Management and Budget, Special Analysis K, 1985

Figure 6.

ESTIMATED RATIOS OF NON-DEFENSE R&D EXPENDITURES TO GROSS NATIONAL PRODUCT (GNP) FOR SELECTED COUNTRIES



Source: SCIENCE INDICATORS: THE 1985 REPORT, National Science Board, National Science Foundation, 1986

Fig. 7 • An encouraging trend, however, has been the increasing fraction of federal support for *domestic* R and D that is going for basic research. Development expenditures are, quite properly, being left to industry.

The second area of concern is people. The research training pipeline is long, from high school through graduate school, and its size cannot be increased rapidly. There must be a steady flow of young people through this pipeline if our economy is not to be starved of technical advances.

Because of the length of the pipeline, it is long-term trends that count.

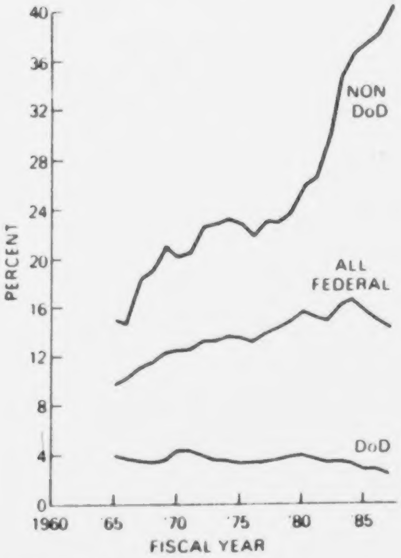
Fig. 8 • Twenty years ago the United States had far more scientists and engineers per capita than any of our competitors. Today that is no longer the case.

Fig. 9 • The proportion of our young people who are attracted to programs in science and engineering reached a peak in the early 1970's, and has since declined.

Fig. 10 • The numbers of students in the appropriate age brackets will decline from now through the mid-1990's. Even if we could continue to attract students at the 1983 rate, this decline means that we would train almost 700,000 fewer graduates in these fields in the next twenty years.

Figure 7.

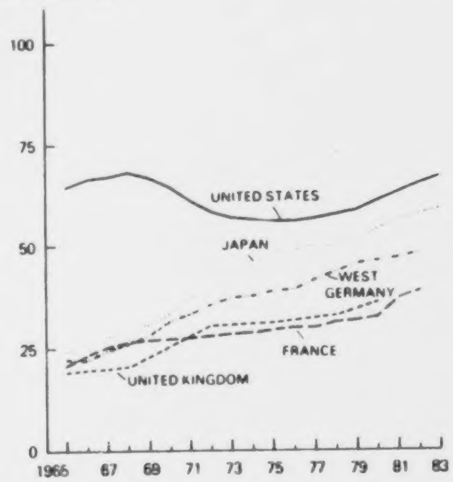
BASIC RESEARCH AS A PROPORTION OF ALL FEDERAL R&D, BY CATEGORY



Source Office of Management and Budget Special Analysis K 1985

Figure 8.

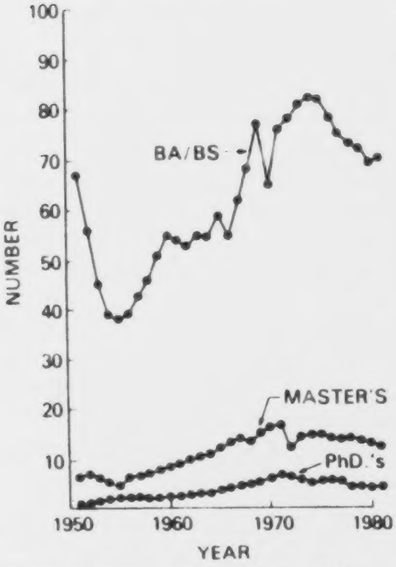
SCIENTISTS AND ENGINEERS ENGAGED IN RESEARCH AND DEVELOPMENT PER 10,000 LABOR FORCE POPULATION BY COUNTRY



Source SCIENCE INDICATORS THE 1985 REPORT National Science Board National Science Foundation 1986

Figure 9.

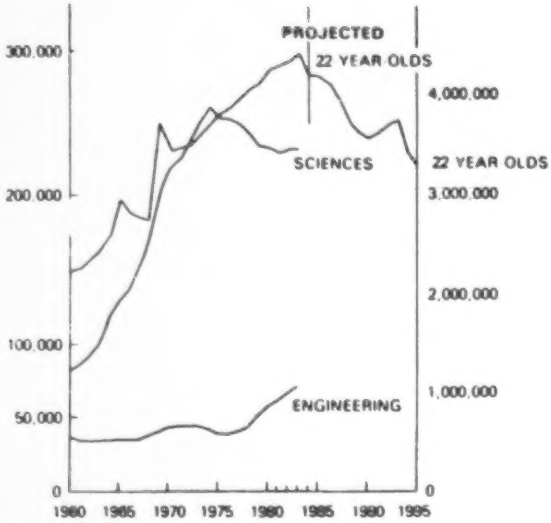
SCIENCE/ENGINEERING DEGREES PER THOUSAND IN APPROPRIATE AGE COHORT, 1950-1981



Source National Science Foundation (extrapolated from data provided by Science Resources Studies division) 1985

Figure 10.

S/E BACHELOR'S DEGREES AND 22-YEAR-OLD POPULATION



Source National Center for Education Statistics and Census Bureau data 1985

- Fig. 11 • With fewer American citizens electing technical fields, we are increasingly dependent on foreign nationals in some of the most important specialties. In recent years more than half of our new engineering PhD's have been foreign nationals. So have a large and rising proportion of mathematicians and physicists - and these are the core disciplines of a technological society.

Accepting foreign students is an American tradition that we must continue, in part because science and scientists are a uniquely good communication link in a troubled world. But it would be a serious mistake to rely *too heavily* on foreign sources for one of our most valuable assets for the future: scientific and engineering talent. Recruiting more of our own women and minorities into science and engineering careers is a good place to start.

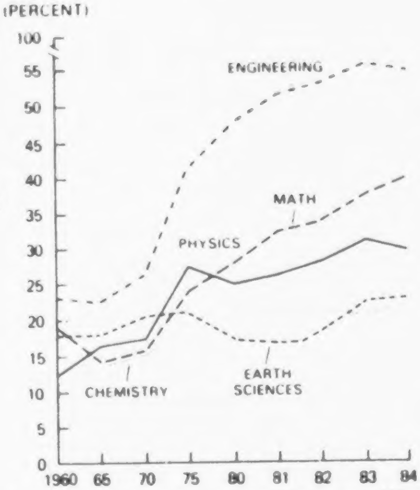
Investment in facilities and equipment is the third requirement for a technologically healthy economy. The United States has the best-developed university system in the world, and extensive facilities in government labs and in industry. Industry has kept pace with needed investment in new facilities and equipment. Government laboratories vary widely. But universities have a serious problem of obsolescence.

- Fig. 12 • In the past twenty years federal investment in university research plant (facilities, land, equipment, etc.) has declined in real terms by 95%.

Figure 11.

DOCTORAL DEGREES AWARDED TO FOREIGN STUDENTS AS A PERCENT OF ALL DOCTORAL DEGREES FROM U.S. UNIVERSITIES, BY FIELD

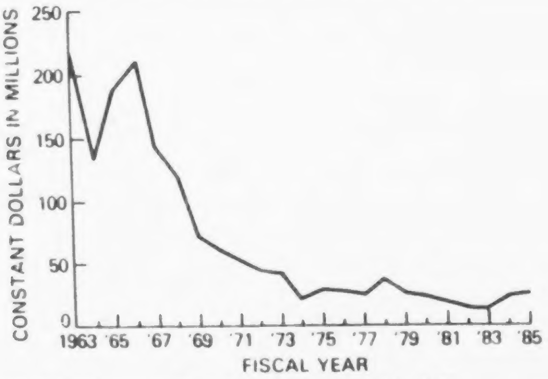
ENGINEERING, MATHEMATICS, AND PHYSICAL SCIENCES



Source: SCIENCE INDICATORS THE 1985 REPORT National Science Board National Science Foundation 1986

Figure 12.

FEDERAL OBLIGATIONS FOR R&D PLANT TO UNIVERSITIES AND COLLEGES



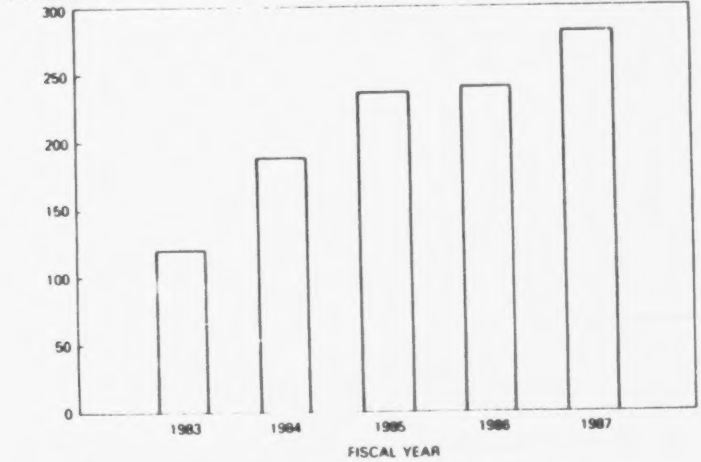
Source: National Science Foundation Division of Science Resources Studies 1985

In response to a growing understanding of the need, federal investment in research equipment began to rise significantly in 1984, when it totalled about $335 million, or 6.2% of total federal R and D support to universities.

Fig. 13
- The National Science Foundation devotes a large and increasing proportion of its resources to research facilities and equipment. In 1987 it will spend almost 17% of its total budget for these purposes. Other agencies are also devoting an increasing fraction of their resources to these purposes - but the total is far from adequate to meet the need.

Figure 13.

INSTRUMENTATION
FY 1983-FY 1987



Source National Science Foundation Office of Budget Audit and Control 1986

Conclusion

While industry and state governments are deeply involved, basic research and education in science and engineering is a well-established responsibility of the federal government. Basic research produces knowledge that is available to all, not just the organization that pays for the research. In many fields it is also too expensive for any single company or state government to sponsor. Thus the nation cannot rely on industry and the states to fund basic research adequately.

Investment in science and engineering research has been the source of much of our economic progress over the past four decades. It continues to be the best single way that we can provide the jobs and national wealth that we must have in decades to come.

Our science and engineering base, however, needs renewed attention. In the past two decades we have not kept up with the need to make steady investments in people, equipment, and facilities. As a result, close inspection reveals many strains and deficiencies. In the modern world that we live and compete in, such inattention to our real needs is fraught with danger.

We must meet the challenge of international economic competition in the decades ahead. Science and technology can provide the means to do it, but only if we find the resources to strengthen our effort markedly. The most basic considerations of national welfare demand that we do no less.

○